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## CONTENTS OF PROCEEDINGS.

Page

XXXVI. On the Objective Reality of Combination Tones. By A. W. RÜCKER, M.A., F.R.S., and E. EDSER, A.R.C.S. (*continued*) ..... 421

XXXVII. Some Acoustical Experiments. By CHARLES V. BURTON, D.Sc., Demonstrator in Physics, University College, London ..... 430

XXXVIII. Tests of Glow-Lamps, and Description of the Measuring Instruments Employed. By Prof. W. E. AYRTON, F.R.S., and E. A. MEDLEY ..... 439

## NAME INDEX (ABSTRACTS).

Altschul, 330.
Angelot, 298.
Bachmetjew, 324.
Bartoli, 316.
Bauer, 314.
Beckmann, 327.
Bedell, 322.
Bergey, 293.
Berthelot, 294.
Billings, 293.
Blondel, 311.
Boltzmann, 313.
Bouty, 326.
Brown-Séquard, 294.
Brühl, 328.
Carrousel, 293.
Charpy, 328.
Le Chatelier, 314, 315.
Chauveau, 317.
Child, 295.
Clève, 294.
Cohn, 320.
Colson, 304.
Cornu, 301.
Crehore, 322.
Dana, 296.
Deslandres, 308.
Eisler, 317.
Elster, 323.
Étard, 310.
Fizeau, 302.
Forch, 315.
Garbasso, 312.
Gautier, 328.
Geitel, 323.
Goldschmidt, 300.
Hamburger, 326.
Hammond, 294.
Hanchett, 319.
Harries, 318.
Hartmann, 301.
Hedin, 327.
Heerwagen, 320.
Heycock, 315.
van't Hoff, 300.
Hopkinson, 318.
Humboldt, 299.
Jaumann, 320.
Kelvin, 323.
Köppe, 326.
Kirchhoff, 313.
Klemencic, 325.
Knietsch, 330.
Kohlrausch, 309.
Kundt, 302.
Lamb, 315.
Langlet, 294.
Lanphear, 295.
Lea, 295.
Leduc, 295.
Leitch, 321.
Liebenthal, 306.
Liévin, 327.
Linebarger, 313.
Lippmann, 309.
Loewy, 296.
Lorentz, 328.
Marchis, 315.
Mazzotto, 320.
McCrae, 315.
Merkel, 294.
Merritt, 310.
Meyer, 315.
Michelson, 302.
Mitchell, 293.
Moëssard, 307.
Moissan, 317.
De Montessus, 303.
Morley, 302.
Müller, 298.
Naccari, 316.
Nagaoka, 324.
Natanson, 303.
Neison, 296.
Neumann, 302.
Nodon, 317.
Olszewski, 303.
Pictet, 313.
Planck, 313.
Poincaré, 306.
Ponsot, 305.
Puiseux, 296.
Quick, 295.
Quincke, 319.
Ramsay, 294.
Raoult, 326.
Riddle, 315.
Riemann, 318.
Rimbach, 309.
Schröter, 296.
Schuster, 306.
Shaw, 321.
Sheldon, 321.
Spring, 326.
Stock, 327.
Stracciati, 316.
Suess, 296.
Teisserenc de Bort, 294.
Tissandier, 293.
Trabert, 323.
Trouton, 313.
Tschermak, 298.
De Vries, 326.
Wadsworth, 318.
Waldo, 299.
Wiess, 318.
Winkelmann, 316.
Wurts, 324.
Zehnder, 301.
Zenner, 315.

sufficient to say once for all that they were adopted in each case.

The main object of the enquiry was to obtain evidence as to the objective reality of the combination-tones, and for this purpose the following experiments were arranged.

Each box of the siren had four circles of holes which could be used separately or together. The number of openings in the upper box were 9, 12, 15, and 16, and in the lower 8, 10, 12, and 18. It will be convenient to refer to these as the 9 row of holes, and so on.

### *Experiment* I.

The 12 and 15 rows of holes in the upper box were opened, and the pitch was raised until the upper note gave slow beats with a fork of 320 vibrations per second. The lower note was then the C of 256 vibrations. The difference or beat tone of 64 vibrations affected the instrument powerfully. The experiment was tried both by night and by day. It was difficult to keep the siren exactly at the true pitch, but when the beats were very slow the bands continually disappeared, sometimes for many seconds at a time, then appeared for a moment and then disappeared again. As soon as the pitch was lost by a few beats per second, the bands remained steady and clearly visible.

The experiment was repeated with the 9 and 12 rows of holes. When the upper note was C of 256 vibrations, the lower note was 192 vibrations. The difference-tone of 64 vibrations affected the fork very powerfully.

The experiment was also modified by opening the 10 and 12 rows of holes. When the notes corresponded to 320 and 384 vibrations respectively, the bands disappeared as before.

### *Experiment* II.

In experiment I. the frequencies of the difference-tone and of König's first lower beat-tone were identical. The experiment was therefore varied by using 8 and 18 rows of holes. The frequency of the difference-tone was thus proportional to 10, while that of König's lower beat-tone would be $18-2\times 8=2$. When the siren was revolving at the rate of

6·4 revolutions per second, the two notes corresponded to 51·2 and 115·2 vibrations per second, the difference-tone being 64.

The pitch was determined on different occasions by different methods. Firstly, by noting the beats between the higher note and a König's fork adjusted to 115·2 complete vibrations per second, and secondly, by watching the row of eight holes through a slit carried by a fork which gave 25·5 vibrations per second.

The effect was rather feebler than in the last experiment, but there was absolutely no doubt as to the objective reality of the difference-tone. The bands regularly disappeared when the required pitch was obtained, and reappeared when it was lost.

Again the 15 and 9 rows of holes were used. The difference-tone is thus proportional to 6, and König's beat-tone to $9 \times 2 - 15 = 3$. When the rate of revolution was $10\dot{6}$ the two notes were 160 and 96 respectively. In this experiment the mirror-resonator which responds to 161 vibrations was employed to determine the rate of the siren. The bands and the spot of light were sometimes watched together: on another occasion one observer who could not see the bands raised his hand whenever the spot of light moved. The bands invariably disappeared at the instant that this signal was made.

## *Experiment* III.

The next experiment was directed to determine the objective reality of König's lower beat-tone when the interval was greater than an octave. The 8 and 18 rows of holes being kept open as before, the speed was increased until the lower note was that of 256 vibrations. The upper note was then 576, and König's lower beat-tone was of $576 - 2 \times 256 = 64$ vibrations.

We lay less stress on negative than on positive results; but we tried for a long time on two occasions to get evidence of the objective character of the note, but entirely failed. The pitch was determined by the beats with a 256 fork.

*Experiment* IV.

We next turn to observations on the summation-tone. The 8 and 10 rows of holes were opened, so that when the cover made 3·55 revolutions per second the summation-tone would be that of $18 \times 3{\cdot}\dot{5} = 64$ vibrations.

The pitch of the notes given by the siren was again determined in different ways on different occasions. The summation-tone being produced in the lower box, the 15 row in the upper box was also opened, thus producing a note of $15 \times 3{\cdot}\dot{5} = 53{\cdot}\dot{3}$ vibrations per second. The required speed was determined by making the beats vanish between this note and a König's fork tuned to give 53·3 vibrations. With this method it was difficult to keep the speed constant for a length of time sufficient to disturb the resonating fork appreciably. When the pitch was altered very slowly the bands disappeared just as the right note was reached, and did not disappear at any other time during the experiment.

On another occasion the 9 and 12 rows of holes were opened, so that the summation-tone of 64 vibrations would be given when the siren made 3·05 revolutions per second. The 18 row of holes was watched through a fork of 27·2 vibrations, so that 54·4 views would be obtained while a hole moved over $18 \times 3{\cdot}05 = 54{\cdot}9$ intervals. Hence the right pitch was obtained when the holes moved slowly forwards. The bands invariably disappeared when this state of things was attained.

On a third occasion the lower cover of the siren was covered with a thin piece of silvered glass as above described, carrying a concentric circle of black paper, the edge of which was divided into 18 equidistant cogs. An image of these was produced on a screen by a lens, and made intermittent by the 27-vibrations fork. The disturbance due to the summation-tone was again and again made evident when the images of the cogs appeared to be moving slowly. In the intervals the bands were beautifully steady.

The earlier of these experiments were performed before, and the later ones after, the apparatus had been taken down and set up again in another room. They left in the minds of those who saw them no shadow of doubt as to the objective reality of a note corresponding in frequency with the summation-tone.

We now turn to experiments intended to throw light on the cause of the production of this note.

*Experiment* V.

It has been suggested that the summation-tone may be the difference-tone of partials. König (*Acoustique*, p. 127) remarks that it may occasion some surprise that the particular harmonics whose difference-tone corresponds to the summation-tone should be especially prominent; but he points out that in some cases the difference-tones of the lower harmonics correspond either to the fundamentals or to some of their upper partials. In the case of the fourth (3 : 4), however, König remarks that the 5th partials would give a difference-tone (5) which could be distinguished from the lower partials, and that the difference-tone of the 7th partials would give the summation-tone. Now we have already proved (Exp. IV.) that the summation-tone produced by two notes separated by the interval of a fourth (9 : 12) is objective; and if this is due to the difference-tone of the 7th partials, there seems to be no reason why the difference-tone of the 5th partials should not be objective also, and probably more intense.

We therefore increased the velocity of revolution to 4·27 per second, the 9 and 12 rows of holes being opened as before. The frequencies of the two notes were thus 38·43 and 51·24. The pitch was determined by keeping the 12 holes nearly stationary when viewed 51 times a second by aid of the 25·5 fork. The first difference-tone was 12·81, and the difference-tone of the 5th partials was 64·05. When the speed corresponding to this difference-tone was attained there were occasional flickers of the bands, so that it is possible that it has an objective existence. But, on the other hand, the effect was less than that produced by the summation-tone. The bands never disappeared for any considerable length of time, as they did when the fork responded to the summation-tone, and the experiment left no doubt in our minds that the greater effect was produced by the summation-tone.

*Experiment* VI.

The same point was also investigated in another way. If the summation-tone of two notes of frequencies $p$ and $q$

corresponds to the difference-tone of the $n$th partial, we must have

$$(p+q) = n(p-q),$$

where $n$ is an integer. If, however, the 9 and 16 rows of holes were opened,

$$p+q=25, \quad p-q=7;$$

so that the summation-tone could not be produced by partials of the same order. The 10th partial of the higher note beating with the 15th of the lower note ($160-135=25$) would indeed have the same frequency as the summation-tone, but it appears to us absurd to suppose that so improbable a combination should produce appreciable results. It is true that lower partials may give beat-tones near to the summation-tone. Thus $5\times 16-6\times 9=26$. But if we are to assume that any pair of partials can thus produce objective tones, the number of combinations will be so great that the fork ought to have been disturbed frequently when the note of the siren was being raised to the required pitch. As a matter of fact, when once the C of 64 vibrations was passed, so that all the partials were higher than the pitch of the resonating fork, no such disturbances were ever observed except when the difference- or summation-tone of the primaries was produced. Putting, therefore, all such fantastic combinations aside, the experiment may be regarded as a test whether the summation-tone can be produced when it cannot be due to two partials of the same order.

When the velocity of revolution was 2·56 per second, the 16 and 9 holes gave notes of 40·96 and 23·04 vibrations. The sum of these is 64. The 12 holes were viewed through a slit alternately closed and opened by a fork of 15 vibrations per second, and when the holes appeared to move slowly the summation-tone caused the bands to disappear.

In this experiment, however, the third partial of the lower note corresponds to 69·12 vibrations, and we thought it desirable to make sure that the disturbance attributed to the summation-tone was not in reality due to this partial. This was the more important, because the difference in the speeds of the siren when the summation-tone and the partial in question corresponded to 64 vibrations was very small.

Thus, when the speed was 2·56 revolutions per second each of the 12 holes would advance through 30·72 intervals in a second, and since the fork gave 30 views per second the holes would appear to move slowly forwards.

When the speed was 2·37 revolutions per second the third partial of the lower note (9 row of holes) would be $3 \times 9 \times 2 \cdot 37 = 64$, and each hole of the 12 rows would advance through 28·44 intervals—that is, would appear to recede through 1·56 intervals per second. Thus the partial would be most efficient in promoting disturbance when the holes appeared to go backward with moderate speed.

The question to be answered was whether these two disturbances could be confused with each other.

When care was taken to keep the pressure in the wind-chest the same whether one or both sets of holes were opened, the effect of the partial produced by the 9 set of holes could hardly be detected. The bands were shaken a little when the row of 12 holes appeared to move backwards, but they did not disappear ; whereas they were completely wiped out by the summation-tone when the two notes were sounded.

When the pressure on the wind-chest was increased, the rate of revolution being nevertheless maintained constant by pressing lightly on the axle of the siren with a straw, the effect of the partial was more marked, but it was always produced when the holes appeared to move backwards.

On the other hand, when both notes were sounded together and when the pitch was gradually *reduced* to the desired point, the disturbance always began when the holes moved slowly forwards. If the pitch fell very slowly it was possible to note a reduction of the disturbance, followed by an increase when the holes appeared to move backwards.

We thus convinced ourselves that the effects of the two sources of disturbance could be distinguished, and that the supposed summation-tone was not due to the partial of the lower note.

## *Experiment* VII.

We have also succeeded in demonstrating the reality of the summation-tone with a mirror-resonator constructed by Professor Boys to respond to a vibration-frequency of 576,

The rows of 15 and 12 holes being opened, notes of 320 and 256 vibrations were produced. When they were sounded separately, the mirror moved slightly. When they were sounded together, the spot of light was driven off the scale when the upper note coincided with that of a 320-vibration fork, but immediately returned when this pitch was lost.

The experiment was varied by using the 16 and 12 rows, and also the 16 and 9 rows. The summation-tone corresponds to 576 vibrations when the upper note is of 329·15 and 360 vibrations in these two cases respectively. The 320-fork was used, and the disturbance occurred in the one case when the pitch of the note was nearly the same as before, and in the other when it was about a tone higher.

We attach great importance to this corroboration of our results by an instrument of a totally different construction from that first employed.

The attempt to obtain proof of the existence of a difference-tone by means of the mirror-resonator of 161 vibrations has not been successful. The instrument is much less affected by the note to which it responds than is that which answers to 576 vibrations, even when that note is produced directly by the siren. It is, therefore, perhaps not wonderful that it gives no reliable evidence of the existence of a difference-tone.

We now sum up the results we have obtained in two tables.

Instrument. Interference Resonator.

| Number of holes in siren. | Interval. | Frequencies. | | Combination-tone. | | König's Beat-tone. |
|---|---|---|---|---|---|---|
| 12 and 10 | Minor Third. | 384 | 320 | 64 | Difference-tone. | 64 |
| 15 „ 12 | Major Third. | 320 | 256 | 64 | | 64 |
| 16 „ 12 | Fourth. | 256 | 192 | 64 | | 64 |
| 15 „ 9 | Major sixth. | 160 | 96 | 64 | | 32 |
| 18 „ 8 | Octave and major tone. | 115·2 | 51·2 | 64 | | 12·8 |
| 10 and 8 | Major Third. | $35\dot{5}$ | $28\dot{4}$ | 64 | Summation-tone. | |
| 12 „ 9 | Fourth. | 36·57 | 27·43 | 64 | | |
| 16 „ 9 | Minor Seventh. | 40·96 | 23·04 | 64 | | |

Instrument. Mirror Resonator.
Summation-Tones.

| Number of holes in siren. | Interval. | Frequencies. | | Sum. |
|---|---|---|---|---|
| 15 and 12 | Major third. | 320 | 256 | 576 |
| 16 „ 12 | Fourth. | 329·15 | 246·85 | 576 |
| 16 „ 9 | Major sixth. | 360 | 216 | 576 |

*Negative Results.*

We have tried several times to obtain indications of combination-tones when the primary notes were produced by tuning-forks. Two of König's large forks adjusted to 48 and 112 vibrations produced no effect when bowed simultaneously before the collector, and, as has been stated, smaller forks giving 256 and 320 vibrations have been placed inside the collector when sounding loudly. No effect whatever was produced, and there can be no doubt that if objective combination-tones are produced in such cases they are very much less intense than those generated by the siren.

Experiments have been made with reeds and with organ-pipes, but up to the present with uncertain results in the first case and negative results in the second. We hope to investigate these cases further.

We have made several attempts to detect combination-tones of higher orders, such as $2p+q$ and $2p-q$, but without success.

*Conclusion.*

We may in conclusion refer to some of the suggestions which have been made to account for the combination-tones by theories other than those of Helmholtz.

König's suggestion that they are the beat-tones of upper partials has been discussed and shown to be inadequate to explain the facts of observation.

Again, it has been argued that the summation-tone is the beat-note between the second partial of the higher note (the octave) and the beat-tone of the two primaries. It follows as

a matter of algebra that such an explanation must always be numerically correct, for $2a-(a-b)=a+b$, and our experiments throw no new light on the matter. It appears to us, however, that since propinquity between the sources of sound, causing a violent disturbance, is favourable to the production of combination-tones, while it is not necessary for the production of beats, the facts of experiment are in this case also in favour of von Helmholtz's views.

A still more subtle objection has been taken by Terquem (*Annales d'Ecole Normale*, 1870, p. 356). When two rows of holes are open in the siren, there may be occasions on which all the holes of both rows are opened simultaneously and others on which only one row is in action at one time. Terquem attempts to calculate the effects of irregularities such as these, but in the first place he specifically refrains from attacking the theory of Helmholtz ; secondly, he does not apply calculation to the siren of Helmholtz ; thirdly, he points out that the relatively large size of the holes in that instrument would reduce the effects he predicts ; and, lastly, he admits that his results require confirmation by experiment. Putting these points aside, however, his theory leads to the conclusion that the two notes which we have been regarding as fundamental are reinforced harmonics in a series of which the fundamental note corresponds to the greatest common measure of these frequencies. Both the summation and the difference tone must be included in such a series ; but Terquem's theory gives no reason why they should have such exceptional importance as experiment proves that they have. Lastly, as he expressly repudiates the idea that partials have an objective existence (*loc. cit.* p. 274), and includes the combination-tones in a series of partials, the experiments described by us must on this point be regarded as opposed to his views.

We think, then, that our experiments prove that von Helmholtz was correct in stating that the siren produces two objective notes the frequencies of which are respectively equal to the sum and difference of the frequencies of the fundamentals, and that our observations are also more or less opposed to the theories by which König, Appun, and Terquem have sought to account for the production of these notes.

We believe that the method we have devised is capable of

greater sensitiveness. It can be extended by employing forks of different pitches, and it is quite possible that less massive forks may enable us to detect effects which have hitherto escaped us. We therefore refrain from any wide generalizations until a wider foundation of experiment has been laid.

P.S.—Since the above was written Prof. S. P. Thompson has drawn our attention to a paper by O. Lummer, published in 1886 (*Verh. phys. Gesell. Berlin*, 1886, No. 9, p. 66), which had escaped our notice, as it is not abstracted in the *Beiblätter*. Herr Lummer obtained evidence of the objective character of the summation-tone by means of the microphone.

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## XXXVII. *Some Acoustical Experiments. By* CHARLES V. BURTON, *D.Sc., Demonstrator in Physics, University College, London**.

### I. *Subjective Lowering of Pitch.*

1. A YEAR or two ago, in the physical laboratory of Bedford College, London, the professor, Mr. Womack, called my attention to the fact that a tuning-fork which was being used by a student appeared to rise perceptibly in pitch as the vibration died away. It then occurred to me that the effect observed might be a subjective one: that one result of increasing the intensity of a musical note without changing its frequency, might be to depress the pitch as estimated by ear. To determine this point I have made experiments upon myself, and on others whose perception of musical intervals was known to be reliable.

2. Let a tuning-fork mounted on a resonance-box be strongly excited by bowing, and immediately let the open end of the resonance-box be brought very close to one ear, the other ear being preferably closed. After a second or so, let the fork and box be moved away from the ear and held at arm's length, then brought close to the ear again, and so on, backwards and forwards, two or three times. If there is any

* Read March 22, 1895.

subjective depression of pitch accompanying increase of loudness without change of frequency, the fork will appear to be lower in pitch when close to the ear and higher when further away, the effect becoming less marked as the vibration gradually dies down. This is what observation shows to be actually the case, and it is evidently nothing of the nature of a Doppler effect with which we are here concerned. In the first place, the motion of the fork from one position to the other may be made quite slow; and in the second place, the difference of pitch is observed, *not* between the fork approaching and the fork receding, but between the nearer and further positions of the fork.

3. In some of these observations a fork giving the note $c'$ (256 complete vibrations per second) has been used, the effect with forks of higher pitch being less marked. When the vibration was made as strong as possible by means of bowing, the lowering of pitch was found to amount to a full semitone (15 : 16). Mr. Womack, Prof. Ramsay, and Dr. G. F. McCleary, who repeated the experiment, all agreed pretty nearly with my estimate: in each case the greatest apparent flattening observed was as much as a semitone.

With a large fork giving *c* (one octave lower) the effect was more striking. In this case the fork with its resonance-box was too unwieldy to be moved bodily to and fro, so it was allowed to stand on a table while the observer's head was lowered so as to bring one ear opposite to the opening of the resonance-box at a distance of a few inches. Thus the fork was heard with great intensity, while on raising the head through half a metre or so the loudness was greatly diminished. Here the lowering of pitch due to the greatest intensity of vibration which I could excite in the fork amounted usually to a minor third (5 : 6) or even more. Thus, repeating the experiment on different days, I have estimated the interval sometimes as rather more and sometimes as rather less than a minor third. Mr. Womack heard something between a minor and a major third; Professor Ramsay a full tone; Dr. McCleary more than a minor third.

4. Before saying more concerning the observations, it will be convenient to mention a physiological theory by which I have attempted to account for the observed effects. Helm-

holtz's discussion of the vibration of the basilar membrane in the cochlea is applicable only to infinitesimal amplitudes, inasmuch as all his equations of motion are linear, and though the introduction of terms of higher order would have made the investigation very lengthy and an exact solution impossible, it is not difficult to see on general grounds what modification of the sense of pitch is to be expected from increased loudness. If the basilar membrane may be regarded as being stretched with a finite tension in the direction of its breadth, and as having no appreciable tension in the perpendicular direction, then Helmholtz shows that it will be vibrationally equivalent to a series of strings stretched side by side and unconnected with one another. For shortness, I shall speak of the membrane as if it actually consisted of such separate strings, and thus, following Helmholtz's theory, we are to suppose that a disturbance of given period reaching the ear excites the strongest resonance in those strings whose natural period is most nearly the same. Now when a string is vibrating freely with finite amplitude, the period of its vibrations is shorter than if the amplitude were infinitesimal; and we are accordingly led to enquire whether a periodic force of considerable intensity would not excite the maximum resonance in those strings whose natural period *for some finite vibration-amplitude* was most nearly the same.

5. As sufficiently representative of our case we may take a system with one degree of freedom, in which the positional force contains a term proportional to the cube of the displacement from equilibrium, as well as one proportional to the first power*, the equation of motion being accordingly written

$$F = m\ddot{x} + k\dot{x} + hx\left(1 + \frac{x^2}{a^2}\right), \quad . \quad . \quad . \quad . \quad (1)$$

where F is the external force impressed on the system, $x$ is the displacement from the equilibrium position, and $m, k, h, a$ are real constants.

* Terms of even degree would imply that the free vibrations were not symmetrical with respect to the equilibrium position, and would therefore be absent in the case of a stretched string, which we have supposed to agree pretty nearly with the physiological case.

If we take F to be a simple harmonic function of the time whose character has been maintained long enough for the whole motion to have become periodic, the value of $x$ will be expansible in a Fourier's series in which the constant coefficients have to be determined. But the expressions thus obtained are very unwieldy; and it will therefore be more convenient to treat $x$ instead of F as a simple harmonic function of the time. It is not easy to say definitely which assumption corresponds most nearly with the actual case, and from what follows I think it will be evident enough that whichever case we consider the general conclusions would be much the same.

6. If we put, then, $x = \mathrm{B}\cos pt,$

the expression for the impressed force is

$$\mathrm{F} = -\mathrm{B}mp^2\cos pt - \mathrm{B}kp\sin pt + \mathrm{B}h\left(\cos pt + \frac{\mathrm{B}^2}{a^2}\cos^3 pt\right);$$

so that

$$\frac{\mathrm{F}}{\mathrm{B}} = \left\{-mp^2 + h\left(1 + \frac{3}{4}\frac{\mathrm{B}^2}{a^2}\right)\right\}\cos pt - kp\sin pt + \tfrac{1}{4}h\frac{\mathrm{B}^2}{a^2}\cos^3 pt.$$

Let $\mathrm{F}_1$ denote the first harmonic term of F (that is, the sum of the terms in $\cos pt$ and $\sin pt$), while $\mathrm{F}_3$ denotes the term in $\cos 3pt$. Then, since

$$\dot{x} = -\mathrm{B}p\sin pt,$$

we have

$$\frac{\mathrm{F}_1^2(\mathrm{max.})}{\dot{x}^2(\mathrm{max.})} = \frac{1}{p^2}\left\{-mp^2 + h\left(1 + \frac{3}{4}\frac{\mathrm{B}^2}{a^2}\right)\right\}^2 + k^2.$$

When $m$, $k$, $h$, $a$, and B are given, the minimum value of the right-hand side corresponds to

$$mp^2 = h\left(1 + \frac{3}{4}\frac{\mathrm{B}^2}{a^2}\right) > h.$$

Also

$$\frac{\mathrm{F}_3^2(\mathrm{max.})}{\dot{x}^2(\mathrm{max.})} = \frac{1}{16p^2}h^2\frac{\mathrm{B}^4}{a^4},$$

which diminishes continually as $p^2$ increases. Hence in general terms we may say that to produce a given amplitude of velocity requires the least amplitude of impressed periodic

force when the periodic time of the force is *somewhat shorter* than would be the natural period of the system for infinitesimal vibrations if the frictional term were abolished. And returning to the case of our strings, we may infer that a similar result will hold good. Hence a force of given finite amplitude will excite in a given string the greatest amplitude of velocity when the period of the force is somewhat shorter than the "natural" period of the string, and hence when a periodic disturbance of finite amplitude reaches the ear, the string whose resonance is excited most strongly will have a "natural" period *longer* than the period of the disturbance.

7. For example, let the note *c* be sounded with very small intensity, and let that part of the basilar membrane which responds most strongly be called the *c*-string. If then the same note *c* is made to sound close to the ear with considerable intensity, the strongest resonance will be excited no longer in the *c*-string, but in some string of longer natural period, such as the B♭-string. Now according to Helmholtz's theory of sound-perception our estimate of pitch depends entirely on localization of the most strongly agitated portion of the basilar membrane, so that in the case just considered, when the note *c* was made to change from a very small to a very much greater intensity, we should expect to hear not only an increase of loudness, but also a lowering of pitch.

8. At the same time it is not to be supposed that the agitation of the "*c*-string" is lessened when the intensity of the note *c* is augmented. The effect must necessarily be in the other direction, so that such a note powerfully sounded close to the ear must perceptibly excite a tract of the basilar membrane corresponding to a considerable *range* of pitch (the place of maximum disturbance being probably not far from the middle of this range).

9. We must even suppose that an increase in the intensity of a note without change of frequency causes the "range of stimulation" (as we may call it) to extend to patches *a little higher* than before, as well as to those a good deal lower.

10. In connexion with § 8 it may be remarked that, when the ear is kept close to the resonant cavity of a strongly-vibrating *c*-fork, the impression of pitch obtained is far from being very definite. One pitch or another within an appre-

ciable range may be heard by a mere effort of attention. On the other hand, a more definite impression is obtained on alternating between smaller and greater intensities of the same note. Thus, on raising or lowering the head, as mentioned in § 3, the interval between the soft note and the loud note may appear at first to be about a minor third, when the fork is sounding strongly. As the amplitude of vibration dies down, the interval diminishes, and it is possible to say pretty definitely when it is just a whole tone, and when it is only a semitone; until, as the note in either position of the head becomes nearly inaudible, the apparent difference of pitch is obliterated.

11. If the orifice of the ear remote from the fork is left open, the sound reaching that ear will be less intense than is heard by the other ear, and the corresponding pitch will be higher. Though of course beats are entirely absent*, it might be thought that two distinct pitches would be heard simultaneously, but this requires a distinct effort of attention. The general impression is of a pitch intermediate between those which would be heard by the two ears separately, so that on closing the further ear a distinct lowering of pitch takes place. For this reason the effects described in § 3 are most marked when the less stimulated ear is kept closed.

12. I have not yet mentioned an effect noticed by an observer in whom hearing with one ear was not normal. With the less sensitive ear the usual effect was reversed; that is, on bringing the ear close to the resonance-cavity of the *c*-fork, the pitch, instead of falling, appeared to rise by about a semitone. Even taking § 9 into account, this result seems rather anomalous.

13. Though variations of pitch accompanying variations of loudness must frequently have been observed, the physiological influences at work do not appear to have been suspected †.

* In the discussion Prof. S. P. Thompson suggested that, if the observations were valid, beats should occur between the notes heard by the two ears; but a consideration of the physical conditions will show that nothing of this kind is to be expected.

† For example, Lord Rayleigh mentions ('Theory of Sound,' 2nd ed. vol. i. § 67) that "tuning-forks rise a little, though very little, in pitch as the vibration dies away."

And yet these subjective influences must be by no means negligible in the case of wind-instrument players ; and even from players of stringed instruments I think I have heard that it is easier to judge of another player's intonation than to be quite certain about one's own. On the other hand, as regards the tuning of the intervals of consonant chords, it must be remembered that the actual criterion is usually the obliteration of beats, so that on this point the judgment is not disturbed by a small subjective uncertainty in the estimation of pitch. I have also convinced myself that, when harmonic upper partials are present, the sense of tonality is largely dependent upon them ; and harmonics from their higher pitch and generally feebler intensity will suffer less subjective repression of pitch than the fundamental.

## II. *Objective Demonstration of Combination-Tones.*

14. When two notes differing in frequency are powerfully sounded in the neighbourhood of one another, secondary tones are produced, of which the most prominent have frequencies respectively equal to the sum and difference of the frequencies of the parent tones. It has been maintained by some writers that these secondary tones are purely subjective, and have no existence external to the ear, and the following experiment was designed to show that in some cases at least the first difference-tone has a real physical existence. If the vibration of the air external to the ear has really a component of the frequency in question, we must suppose this component to have arisen from a failure of the principle of superposition, that is, from the circumstance that as the parent vibrations are of considerable amplitude, the equations of motion cannot be regarded as sensibly linear ; and accordingly it is to be expected that if two sources of sound are brought closer together, the intensity of the difference-tone will increase. I have used two stopped organ-pipes of white metal giving the notes $e'$ and $g'$, the difference-tone being consequently C. The pipes are connected to a well-weighted organ-bellows by flexible rubber tubes, and the distance between them can be varied at will from a few feet to a couple of inches (the walls of the two pipes being then in contact). It is best for the listener to be stationed in a distant room, so that the sound

which reaches him is only of moderate intensity, while the comparatively small distances through which the pipes are moved does not appreciably affect the sounds which they individually send to his ear. The bellows being filled, the sounding pipes are held alternately far apart and near together, and each time, as they nearly approach one another, the difference-tone $C_1$ is heard to boom out with greatly increased intensity. From this, I would suggest, we are to infer that the difference-tone has a real objective existence.

(Further experiments have *not* confirmed this result. Even Prof. Rücker's very sensitive arrangement of interference-bands failed to show any objective difference-tone from the two organ-pipes, suitably tuned. An attempt will be made to deal with the subject more fully in a subsequent paper.)

## Discussion.

Mr. Edser mentioned that Dr. Burton had suggested an explanation of the production of objective tones in the use of the siren which depends on the production of the tones in the wind-chest of the instrument itself, when two rows of holes are simultaneously opened. They had made an experiment which seemed to show that the above explanation was incorrect, for on connecting together the wind-chests of two sirens, fixed on the same spindle, by means of a short length of wide metal tubing, no effect was observed on the bands when the two notes were produced on different instruments having what was practically a common wind-chest.

Prof. Everett (in a letter) said he considered the experiments described in the paper proved conclusively the objective existence of the summation-tones as distinguished from the supposed beat-tones. He had lately been investigating the pitch of the loudest combination-tone obtained when two notes having frequencies as 3 to 5 are sounded. Is the frequency of this tone 2, *i. e.*, the first difference-tone, or is it 1, which corresponds to the first term of the Fourier series for the periodic disturbance? In the chords 2 to 3, 3 to 4, 4 to 5, &c., the difference of the two integers being unity, the first difference-tone is identical with the first Fourier

tone. Where the difference of the two integers which express the chord is not unity, then the writer considers that experiments he has made with strings and pipes show that the first Fourier term is usually the only combination-tone that is audible.

Prof. S. P. THOMPSON considered that care should be taken to define what we mean by the subjective or objective existence of a note, and recommended the use of the term "ear-made tone." There are two very delicate methods which have already been employed for detecting the existence of a given note in the air :—1. The formation of ripples on a soap-film stretched over the opening of a resonator tuned to the required pitch (Sedley Taylor). 2. The sounds produced in a telephone connected to a microphone placed on a thin elastic membrane stretched oves the neck of the resonator (Lummer). It was very important to limit our acceptance of the demonstration of the objectivity of combination-tones given by the authors of the paper to the case actually proved, *i. e.,* to tones produced by the polyphonic siren ; it did not necessarily follow that if pure tones produced by tuning-forks were used, the same results would be obtained. A number of experiments had been made by Zantedeschi in 1857, in which two notes were sounded, and skilled musicians were asked to record their impression of the third tone present. In 75 per cent. of the cases the note recorded was the difference-tone. In the remaining 25 per cent. it corresponded to König's beat-tone. König himself had never heard the summation-tone in the case of lightly bowed forks. Voigt, in a theoretical paper, has shown that if there are two disturbances, whose mean kinetic energy differ, the Helmholtz tones well be produced ; but that if the mean kinetic energy of the two disturbances are equal, the Helmholtz effects soon die out, and you get beat-tones or beats. He (Prof. Thompson) considered that Dr. Burton had allowed his perception of tone to be governed by the quality of the note, and that the apparent lowering of pitch was due to the variation in the intensity of the overtones present.

In reply to Prof. Thompson, Dr. BURTON said he did not merely perceive a lowering of pitch, but he was able to

estimate the change in pitch and say at what instant, as the vibrations of the fork died out, the lowering amounted to a tone or half a tone, &c.

Mr. BOYS said he found that by careful attention he could apparently persuade himself that the note in Dr. Burton's experiment was lowered or raised in pitch, or that it remained unaltered. A similar effect in the case of the eye could be obtained with a stereoscopic picture.

The CHAIRMAN considered that while Helmholtz's explanation of the production of combination tones might be real, it did not follow that this explanation gave the sole cause of their formation. In particular Helmholtz does not explain why the tones should only be produced by some sources of sound.

Prof. RÜCKER, in his reply, said he did not deny the existence of König's beat-tones; in fact, he had heard them. They did not lay much stress on the negative result of the experiment they had made to test the objective existence of these beat-tones.

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## XXXVIII. *Tests of Glow-Lamps, and Description of the Measuring Instruments Employed. By* Prof. W. E. AYRTON, *F.R.S., and* E. A. MEDLEY*.

THE conditions under which the glow-lamp can be most economically used have attracted considerable attention since a paper entitled "The Most Economical Potential Difference to employ with Incandescent Lamps" was read before the Physical Society in February 1885 † by Professor Perry and one of the authors of the present communication. In this paper they stated that it was well known, from experiments made by their students in 1880, and from results published in 1881 by Lord Kelvin, then Sir William Thomson, as well as from subsequent experiments, that the light obtained from an incandescent lamp increased much more rapidly than the power expended in it; or, that the number of

* Read December 14, 1894, and January 25, 1895.

† Proc. Physical. Soc. vol. vii. p. 40; Phil. Mag. April 1885, p. 305.

candles produced per watt of power expended in the lamp increased as the filament became hotter. But it was pointed out in the paper in question that such experiments by themselves gave no idea of the commercial value of any particular glow-lamp, because they afforded no indication of its life when run at different efficiencies, and it was known that the length of time a filament would last was the less the higher its temperature.

They then proceeded to show that the cost of lighting per hour per candle could be divided into two parts, viz., the cost as to lamp renewals and the cost as to power, the first being equal to the price of a lamp divided by the product of its life into its candle-power, and the second to the price of one watt-hour multiplied by the watts per candle. So that, if $L(v)$, $C(v)$, and $W(v)$ were the life in hours, the candle-power, and the watts per candle respectively expressed as functions of the pressure in volts kept constantly on the lamp, and if $p$ were the price of a lamp and H the price of one watt-hour, then the cost per hour per candle equalled

$$\frac{p}{L(v) \times C(v)} + H \times W(v),$$

and the value of $v$ which made this expression a minimum, they showed, was the proper P.D. to employ with the particular type of lamp.

The authors gave two methods of solving this problem defined by their equation of cost—the one a graphical method, and the other an analytical one. The graphical method consisted in drawing curves from the best experimental results then available to represent $L(v)$, $C(v)$, and $W(v)$ respectively in terms of $v$, and from these three curves finding the values of $L(v)$, $C(v)$, and $W(v)$ for many values of $v$, then, by substitution in the cost equation, data were obtained from which a last curve showing the relation between potential difference and cost per hour per candle could be drawn.

The curves on p. 45, Proc. Phys. Soc. vol. vii. 1885*, showed the results obtained when this method was applied to the case of 16 C.P. 100-volt Edison lamps, using as experimental data the results of tests, made at the Finsbury Technical College,

* Phil. Mag. *tom. cit.* p. 308.

on the relations between candles, volts, and watts, and the values of the life of this type of lamp when run at different pressures, as published by M. Foussat.

The curve AAA showed the cost per candle for lamp renewals during 560 hours when the price of a new lamp was five shillings; BBB the cost per candle for power during the same period, one horse-power for 560 hours being reckoned at £5, and CCC gave the total cost per candle for 560 hours. The ordinate of this latter curve had a minimum at about 101·4 volts.

With the analytical method, on the other hand, empirical equations were first found to represent as nearly as possible the experimental results. Thus it was found that

$$\frac{1}{L(v)+C(v)}=10^{0\cdot07545\,v-11\cdot697}$$

and

$$W(v)=3\cdot7+10^{8\cdot007-0\cdot07667\,v};$$

therefore the total cost per hour per candle equalled

$$p\,10^{0\cdot07545\,v-11\cdot697}+H(3\cdot7+10^{8\cdot007-0\cdot07667\,v}).$$

From this equation, using the same values for the price of lamps and power as before, it was shown that to make the cost a minimum the P.D. maintained between the lamp-terminals should be 101·1; a result in close agreement with that obtained graphically.

At the next meeting of the Physical Society a paper was read by Dr. Fleming *, in which he considered what proportion the cost of lamp renewals should bear to the cost of power in order that the total cost should be a minimum. As the result of the examination of various experiments, he showed that for any particular type of lamp the average life could be expressed as an exponential function of either the watts per candle, or of the candle-power, or of the P.D.; that is, he showed, for the lamps with which he was dealing, that

$$L=\frac{A}{W^{\alpha}}=\frac{B}{C^{\beta}}=\frac{1}{v^{\gamma}},$$

where L was the life, W the watts per candle, C the candle-power, $v$ the P.D., and A, B, $\alpha$, $\beta$, and $\gamma$ constants for any

* Proc. Physical Soc. vol. vii. p. 55; Phil. Mag. May 1885, p. 368.

particular type of lamp. Then, by substitution in the cost equation already referred to, he obtained the result that for maximum economy the cost of power must be to the cost of renewals as $\alpha\frac{\beta-1}{\beta}$ to unity. The values for $\alpha$ and $\beta$ he gave for Edison lamps as $6\frac{1}{4}$ and $4\frac{1}{6}$ respectively; so that he finally arrived at the result that, whatever the price of lamps, or of electrical energy, the pressure used must be such that the annual cost of renewals should be about 17 per cent. of the total annual cost.

In April 1888, in a paper read before the American Institute of Electrical Engineers, Mr. Howell gave numerous examples of the application of Ayrton and Perry's graphical method to the finding of the efficiency at which various types of lamps should be run to obtain maximum economy for various prices of lamps and power.

He gave a curve showing the results of his tests of Edison lamps when run at different efficiencies, but the lamps he used were so abnormally good, compared with any with which we are acquainted, that his experiments have no practical value. For example, his lamps run at 4·5 watts a candle lasted, he says, nearly 12,000 hours.

Further, Mr. Howell's paper can hardly be said to have advanced the matter beyond the point at which it was left by the considerations contained in the paper on "The Most Economical Potential Difference, &c.," read before this Society in 1885; for he took no account of the changes which more recent experiments had shown to occur in lamps during their life, and which render any results obtained by methods that disregard these changes useless for practical purposes.

Numerous experimenters had found out that there was a serious diminution in the light emitted by a lamp as it grew old, and also a very considerable rise in the watts consumed per candle. Further, there was evidence to show that the shape of the curve between candle-power and time was not the same for different pressures; so that it had come to be recognized that, in order to determine the P.D. at which lamps should be run, it was useless to make a comparison merely between the efficiency of lamps when new and the number of hours they will last at various voltages.

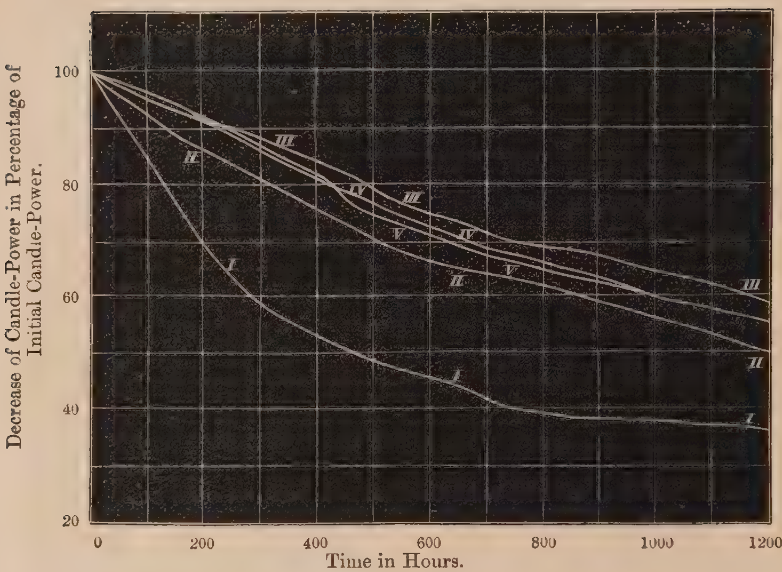
Moreover, this falling-off in the quality of a lamp as it ages led people to consider another point, viz., Might it not be possible, in consequence of this deterioration, for a lamp to be economically dead before the filament had actually broken? For the increased cost of the current required, compared with the light given out, might more than overbalance the expense of replacing the lamp with a new and, therefore, more brilliant one.

Before describing the results which we have obtained regarding this interesting question of the existence of a point, called by the Americans the "smashing point," beyond which a lamp cannot be economically used, it will be well to shortly indicate the general conclusions to which previous experimenters have been led from tests on the modern glow-lamp.

In November 1892 Mr. Feldman published a table compiled from the results of tests made by Prof. Thomas and Messrs. Martin and Hassler in America, by M. Haubtmann in France, and from some of his own measurements, which contained mean values for more than 500 lamps taken from 28 different factories and representing 49 different types. He divided the lamps into five groups according to their initial efficiencies, and gave for each group the average candle-power in per cent. of the initial candle-power and the average watts per candle at every hundred hours in the lives of the lamps. The curves in figs. 1 and 2 are drawn from the figures given in this table, fig. 1 showing the candle-power and fig. 2 the watts per candle as the lamps grow old. The numbers I., II., III., IV., and V. on the curves refer to the initial efficiencies of the lamps whose behaviour the curves illustrate, and, as seen from the table on fig. 1, the curves marked I. are drawn from results obtained from lamps which initially required from 2 to 2·5 watts per candle; for curves marked II. the initial consumption was 2·5 to 3 watts per candle; for curves III. from 3 to 3·5; for curves IV. from 3·5 to 4; and for curves V. above 4 watts per candle.

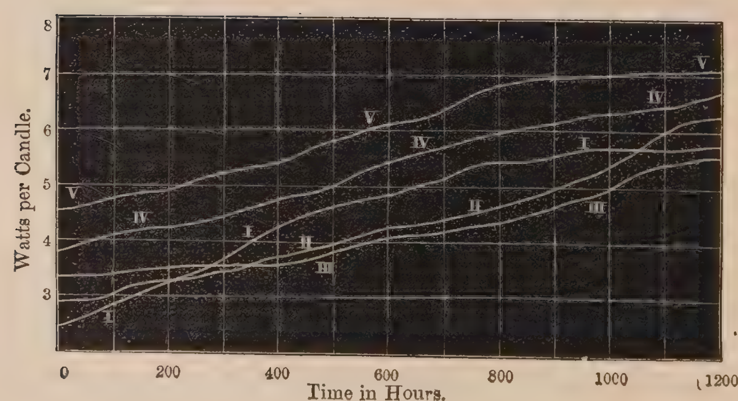
These curves show that the fall in candle-power varied from 40 to 65 per cent. in 1200 hours, or, rejecting lamps having as low an initial consumption as from 2 to 2·5 watts per candle, Mr. Feldman's curves would lead us to expect

Fig. 1.—Tests of American and European Lamps.




| Curves marked | Initial Watts per Candle. |
|---|---|
| I. .................. | From 2·0 to 2·5 |
| II. .................. | From 2·5 to 3·0 |
| III. .................. | From 3·0 to 3·5 |
| IV. .................. | From 3·5 to 4·0 |
| V. .................. | Over 4. |

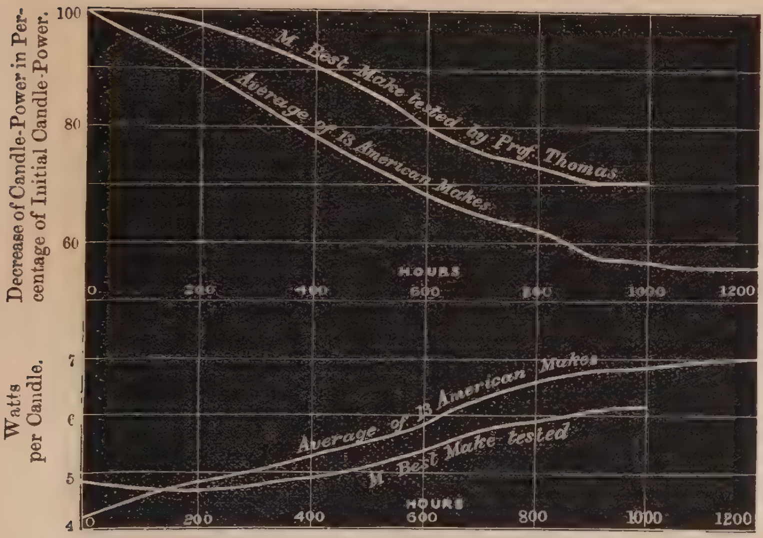
Fig. 2.



that the candle-power of an average lamp would fall about 40 per cent. in 1000 hours.

From the curves marked A in fig. 3, which give the results of Professor Thomas' tests made in 1892 on 127 lamps of 13 American makes, it can be seen that the average American lamp of that date dropped about 43 per cent. in candle-power in 1000 hours, the watts per candle in the same time

Fig. 3.—Prof. Thomas' American Tests.




rising from 4·2 to 6·9. The curves marked M on the same figure show the results obtained from the best make of lamp tested by Professor Thomas; the average of the 10 lamps tested showed a drop of 30 per cent. in candle-power in 1000 hours, and a rise from 4·8 to 6·1 in watts per candle.

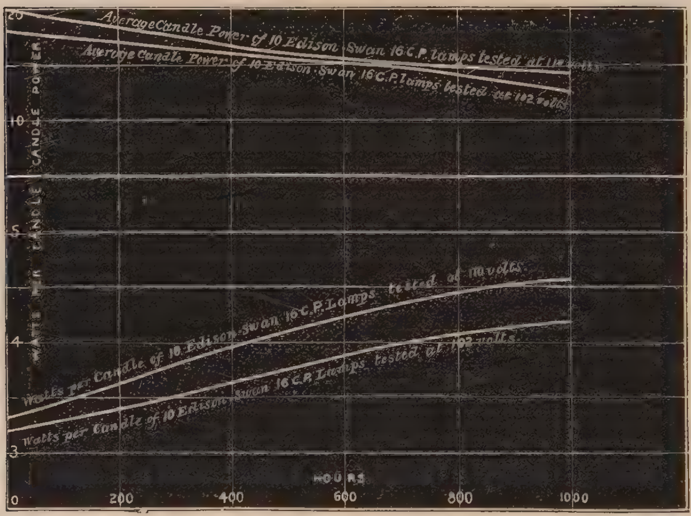
In September 1892 M. Haubtmann published tests on many European makes of lamps. The curves in fig. 4 show the results he obtained from testing twenty 16 C.P. Edison-Swan lamps, ten at 102 and ten at 110 volts. Those run at 102 volts showed a drop of about 30 per cent. in candle-power in 1000 hours, the watts per candle rising in the same time from 3·27 to 4·4; the lamps run at 110 volts in 1000 hours dropped 28 per cent. in candle-power and rose from 3·35 to 4·58 watts per candle.

If, now, in the light of the knowledge obtained from these experiments, we look at the equation of cost quoted in the beginning of this paper, we see that some changes must be made in it before it expresses the truth. As originally given it was :—Cost per hour per candle equals

$$\frac{p}{\mathrm{L}(v) \times \mathrm{C}(v)} + \mathrm{H} \times \mathrm{W}(v);$$

but, as neither the candle-power nor the watts per candle remain constant throughout the life of the lamp, $\mathrm{L}(v) \times \mathrm{C}(v)$

Fig. 4.—Tests by M. Haubtmann on 20 similar 16 Candle-Power Edison-Swan Lamps, 10 of which were run at 110 volts and 10 at 102 volts.



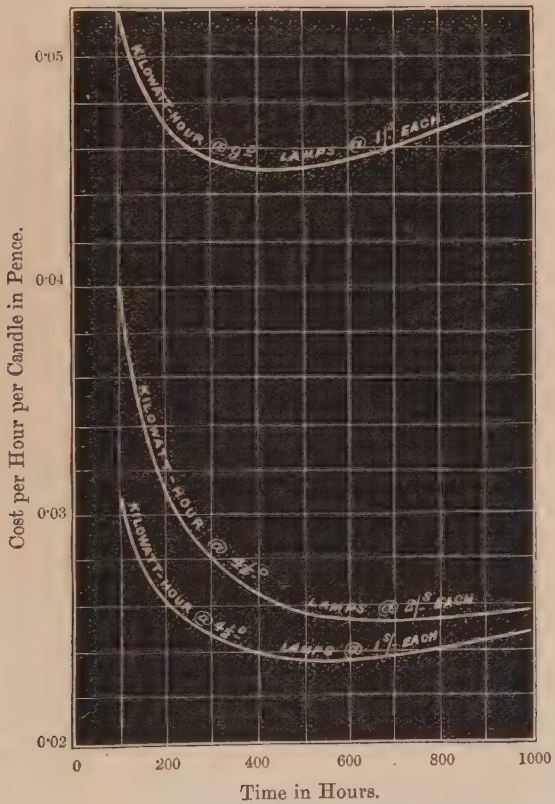

does not give the total candle-hours obtained during the lamp's life, and neither does $\mathrm{H} \times \mathrm{W}(v)$ give the true cost of power per hour per candle. The equation must, therefore, be written :—Cost per hour per candle equals

$$\frac{p}{\text{total candle-hours}} + \mathrm{H} \times \textit{average} \text{ watts per candle.}$$

Exactly what these changes in the equation mean will be best shown by working out the results for a particular type of lamp, and for this purpose we have chosen the American make tested by Professor Thomas, the life curves of which are shown marked M in fig. 3.

It is clear, from these curves, that knowing the initial candle-power, in this case about 12·5, we can calculate at any time during the life of a lamp the total candle-hours obtained and the average watts consumed per candle during that time, and by substituting these quantities in the equation of cost we can find the cost per hour per candle averaged over the number of hours considered.

Fig. 5.—Cost Curves calculated for American make M in fig. 3.




The curves in fig. 5 show the results we have obtained when these calculations are made at different times in the life of a lamp, and for different prices of lamps, and of power. In calculating the figures from which the bottom curve is plotted, we have taken the cost of a new lamp as one shilling,

and the price of a Board of Trade unit as $4\frac{1}{2}d.$, and, as seen, the ordinate of this curve reaches a minimum at about 600 hours, when the cost per hour per candle has dropped to 0·0235 pence. Beyond this point the curve begins to ascend; that is, when a new lamp costs one shilling, and a kilowatt-hour $4\frac{1}{2}d.$, and lamps whose life-histories are truly represented by the curves M in fig. 3 are employed, the cost of obtaining light is least if the lamps are used for 600 hours only, because after that time their diminished quality more than overbalances the cost of renewing.

A change in the price of lamps does not much affect the economical life, as is shown on fig. 5 by the middle curve, which we have calculated for the same price of power but on the assumption that a new lamp costs two shillings. The ordinate of this curve has a minimum value at 650 hours instead of at 600 hours, when a new lamp was supposed to cost one shilling only.

The higher the price paid for energy the more important does this question of economical life become, and the sooner is it necessary to discard lamps, because the cost of renewals bears a less proportion to the total cost. This is illustrated in fig. 5 by the top curve, to obtain which we have assumed that the cost of energy was $9d.$ a unit, and the price of a new lamp one shilling. Now the minimum point falls to 430 hours instead of being at 600, at which it stood when the price of the Board of Trade unit was taken as $4\frac{1}{2}d.$

When cost curves, like those in fig. 5, are drawn for a worse type of lamp, the minimum point becomes more sharply defined and is reached earlier. It might be urged, however, that all such curves merely indicate for how many hours lamps should be used in order that the price paid for the light may be a minimum, but that, if at the end of that time the light given out by the lamps is sufficient for the purpose for which they are intended, surely it would be folly to discard them. The answer to this is, that the lamps employed were of too high candle-power for the necessary illumination, and what should be done is to throw away the nominally higher candle-power lamps that have deteriorated and replace them with new *lower* candle-power lamps.

When, therefore, the special investigation which forms the subject of this paper was commenced at the end of 1892, there was good reason for expecting, first, that with any type of glow-lamp a certain number of hours could be experimentally determined at the end of which it would be economical to discard lamps even when the price of a Board of Trade unit was as low as $4\frac{1}{2}d.$ ; secondly, that it was only economical to run lamps at the pressure marked on them for one particular price of electric energy.

As Edison-Swan lamps were the only lamps that could be legally used in this country in 1892, and as a pressure of 100 volts was one very commonly employed for electric lighting, we decided to experimentally find out what was the most economical pressure to maintain between the terminals of nominal 100-volt 8-candle lamps of this make, and what was the length of their economic lives for different prices of a Board of Trade unit.

As it is well known that the lives of filaments are considerably affected by the steadiness of the pressure between their terminals, it was decided to run the lamps from a battery of storage-cells, and to arrange that the pressure when on the lamps should not vary by as much as one tenth per cent. The capacity, however, of the cells at the Central Technical College which could be entirely set on one side for this investigation and used for running 100-volt lamps from five o'clock every evening to nine or ten o'clock the next morning for five nights of the week, only allowed of nine 8 C.P. lamps being dealt with at a time. The nine lamps were divided into three groups, each containing three lamps, and a perfectly constant, but different P.D. was maintained between the terminals of the lamps in each group.

When our experiments were begun it was known that the price of lamps would fall in the autumn of 1893, therefore it was considered hardly probable that any economy would be gained by running the lamps at less than their normal pressure. Hence we decided to run the three groups at 100, 102, and 104 volts respectively. The tests, however, soon proved that no economy could be gained by running these lamps at as high a pressure as that of 104 volts, and so the

group running at that pressure was changed for one running at 101 volts.

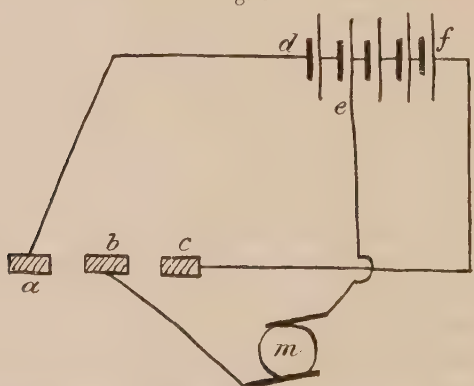
The pressure of the storage-cells which supplied the lamps with current diminished, of course, during the night, so that it was necessary to introduce between the cells and the lamps a variable resistance which could be altered to keep the pressure on the lamps constant within one tenth per cent., and to avoid constant attention it was also necessary that this resistance should be automatically controlled.

As no automatic regulator could be purchased which would keep the pressure throughout the night within one tenth per cent. of the desired value, it was necessary to construct such an instrument.

The apparatus which we employed for this purpose resembled generally the one described in a paper on "The Working Efficiency of Secondary Cells" by Messrs. C. G. Lamb, E. W. Smith, M. W. Woods, and one of the authors of the present communication, see Journ. Inst. Elect. Eng. vol. xix. 1890.

A variable resistance, consisting of four platinoid wires winding on and off a brass roller, was placed in the main circuit between the cells and the lamps. This resistance was geared to a permanent magnet Gramme-motor, the electrical connexions of which are shown in fig. 6. *d e f* was a battery

Fig. 6.




of five storage-cells to the centre *e* of which one brush of the motor *m* was connected, the other brush being joined to the middle cup *b* of a three-way mercury switch *a b c*. The two

outer cups *a* and *c* of this switch were connected with the two ends *d* and *f* of the battery.

It is clear from the figure that the motor revolved one way or the other according as *a* and *b* or *b* and *c* were connected.

A "set up" d'Arsonval galvanometer in series with a resistance was placed across the two points in the main circuit between which it was desired that the pressure should remain constant. To the coil of this galvanometer was attached a pointer ending in a platinum tip which worked between two platinum contacts. The phosphor-bronze strip by which the coil of the d'Arsonval was suspended was twisted several times, so that when the pressure on the lamps was correct the tip of the pointer rested midway between the contacts. If, however, the pressure rose by one tenth of a volt the pointer was deflected and made to touch one of the contacts, and so to complete the circuit of an electromagnet; this, by closing one side of the mercury switch (fig. 6), started the motor, which increased the resistance in the main circuit until the pressure was correct again.

Exactly the opposite happened if the pressure fell, the pointer touched the other contact, the other side of the mercury switch was closed, and the motor working in the reverse direction reduced the resistance between the cells and the lamps.

The pointer was kept from sticking to its contacts by a light wooden hammer which tapped the case of the galvanometer when the motor revolved.

By means of this apparatus it was possible to keep the pressure constant to within at least one tenth per cent.

For many nights the action of this regulator was watched pending the arrival of a recording voltmeter, but the recording voltmeter not being forthcoming, and staying up all night, even for four times a week, interfering much with our work during the day, we decided to construct an automatic check on the automatic regulator.

In series with the first galvanometer was placed a second working in exactly the same way, but with its contacts so far apart that its pointer only touched them for a rise or fall of half a volt in the pressure on the lamps. When for any reason such an alteration took place, the pointer of this second galvanometer, by touching one of its contacts, completed the

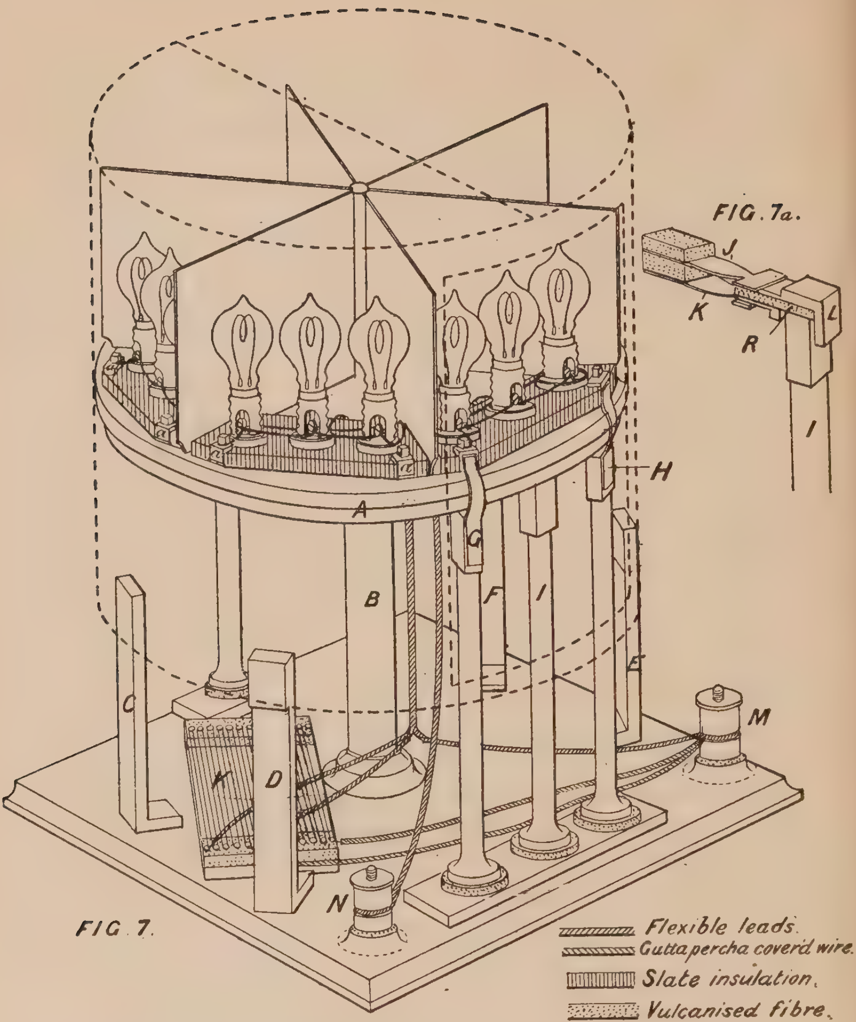
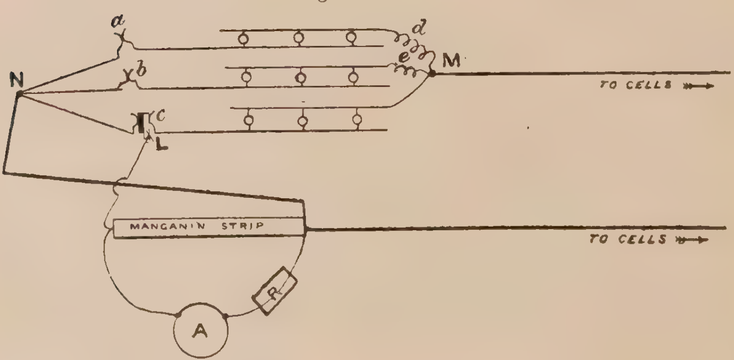



FIG. 7a.

FIG. 7.

Fig. 8.



circuit of an electromagnet; this attracted a piece of soft iron which opened a mercury switch and put out the lamps, and at the same time by stopping a clock indicated the time at which the irregularity took place.

Besides acting as a check on the regulating apparatus, this automatic cut-out worked whenever the pressure rose on a lamp breaking, and so made it possible to tell the exact life of the broken filament.

The lamps being tested were arranged in the special stand shown in fig. 7. The circular plate A, which could be turned on the centre pillar B, was divided into six compartments by the radial divisions shown, and the nine lamps simultaneously tested were placed in three of these compartments, which were coloured a dead black inside. The whole was covered up with an outer tin case, drawn in broken lines, which was fixed to the uprights C, D, E, F, and had an opening in front through which any one of the groups of lamps could be seen when turned into position.

When photometric measurements were not being made, the top of the case was left open to let out the heat.

The arrangements for measuring the current and for bringing it to the lamps are sketched in fig. 8. M and N are the main terminals of the lamp-stand, corresponding with M and N in fig. 7. From M the current, after passing through the resistances *e* and *d*, for the two groups run at lower pressures, went through the lamps and reached the terminal N by means of the copper springs *a*, *b*, and *c*.

When, however, a group of lamps was turned into position for photometric measurements its copper springs *c* (J and K in fig. 7*a*) were separated by a piece of brass L backed with insulation (R in fig. 7*a*), and so the current of the group was diverted through a thick wide strip of manganin, this strip being shunted with an Ayrton and Mather d'Arsonval galvanometer, A, in series with a resistance R, as seen in fig. 8.

This combination of galvanometer, resistance R, and manganin strip was calibrated by direct comparison with a Kelvin balance, and the resistance R was so adjusted that one ampere produced a deflexion of 500 divisions, each of about a millimetre in length. Variations of current during

the experiments could be read to $\frac{1}{5000}$ of an ampere if required, and the current was known accurately in Board of Trade amperes to $\frac{1}{1000}$ of an ampere.

The results of frequent comparisons between an ampere as read off on the direct-reading transparent scale and an ampere as measured with the Kelvin balance, which was also screwed down permanently in position, never showed a difference as large as 1 part in 1500 between September 1893 and August 1894.

The positive and negative terminals of each group of lamps were connected respectively with two squares of brass *a*, *a* (fig. 7) let into the slate bed on which the group was mounted. Two brass springs, G, H, permanently connected with an Ayrton and Mather reflecting electrostatic voltmeter, the motion of the needle of which was damped by moving in a magnetic field, pressed on the brass squares *a*, *a* of whichever group of lamps was brought to the front of the stand.

The zero of the voltmeter was so arranged that the transparent scale, which was direct-reading, was nearly uniformly divided from 50 to 120 volts, each division of about 1·5 millimetre representing 0·2 volt, so that it was easy to read the deflexion to within 2 parts in 10,000.

The electrostatic voltmeter was frequently calibrated, and the maximum change in sensibility between July 1893 and August 1894 did not exceed 0·2 per cent. This change, although small, is fully accounted for by the variation in the zero, since the instrument did not possess the delicate zero adjustment of the later specimens of this type of voltmeter.

The standard of light we used was a two candle-power Methven screen fed with *pentaned* gas, and against this a ten candle-power Bernstein glow-lamp was standardized to give five candles, the Bernstein lamp being employed for the actual comparison with the Edison-Swan lamps.

One of the objects of employing this Bernstein lamp was to test whether or no a convenient and trustworthy standard of light could be obtained by using a low-voltage lamp with its filament at a comparatively low temperature. From this point of view, however, the results were not very satisfactory,

as at the end of some sixty hours of life the lamp had blackened heavily. This secondary standard was of course tested very frequently—as a rule, every time it was used.

For the last few hundred hours of our tests an 8 C.P. 100-volt Edison-Swan lamp running at five candles was used as an intermediate standard of light, and after the first few hours it gave very fair results, the current required to produce exactly five candles dropping in almost a straight line from 0·3893 to 0·3867 ampere in two hundred hours. It is probable, for reasons which will be shortly apparent, that an even better result would have been obtained if the lamp had been run for some fifty hours at its normal brilliancy before it was used as a standard of light.

For measuring the light a Lummer-Brodham photometer was used, and proved itself a very convenient instrument. The lamp-stand was fixed at the end of a three-metre photometer bench, the actual distance between the standard and the Edison-Swan lamps being 319·5 centims.

Some difficulty was found at first in measuring the candle-power owing to the very considerable difference in colour between the lights being compared. This difficulty was considerably reduced by fixing the Bernstein lamp to the spring frame shown in fig. 9, where A is the lamp, S, S are steel springs, and W, W are leaden weights.

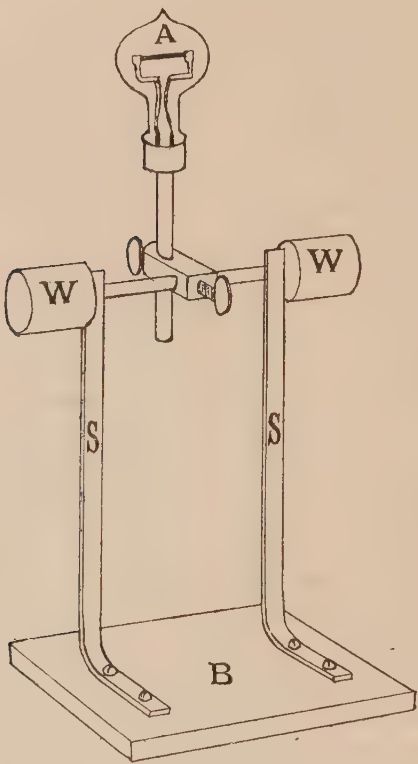
When measurements of the candle-power were being taken, the Bernstein lamp was set gently swinging backwards and forwards in the direction of the bench, and the effect thus easily produced was found to be an improvement on that usually obtained by moving the photometer first a little on one side, and then a little on the other, of the neutral point. After some practice the use of this contrivance enabled us to make measurements of the candle-power which did not differ from each other by more than 0·3 per cent.

The first point noticed after starting the tests was that candle-power, current, and candles per watt all *rose* as the lives of the lamps increased; and as we were not prepared, from the published accounts of experiments previously made on glow-lamps, to expect any such result, we thought it might be due to some error in our measurements. We

therefore made a number of tests to satisfy ourselves that this was not the case.

A current nearly twice as large as the maximum required to be measured was sent through the manganin strip for

Fig. 9.




several hours, but the deflexion of the d'Arsonval galvanometer for a given current was not altered, so that the effect was certainly not due to the heating of the strip or galvanometer-coil.

As the voltmeter was connected with the terminals of the lamp sockets and not with the terminals of the lamps themselves, we tested whether any resistance was introduced by the sockets, or whether any alteration occurred when they

got warm after the lamps had been glowing for some hours. An old lamp was taken and wires soldered to its terminals, and also to the terminals of the socket into which it was screwed, and, by measuring as nearly simultaneously as possible the pressures on the lamp and on the socket terminals under various conditions as to heat and position of lamp, we proved that the resistance of the socket and contacts was never perceptible.

There seemed, therefore, no reason to doubt the results already obtained; but as the accuracy with which the candle-power could be measured had increased with practice and with the introduction of the swinging frame, we decided after 110 hours of testing to start again with new lamps.

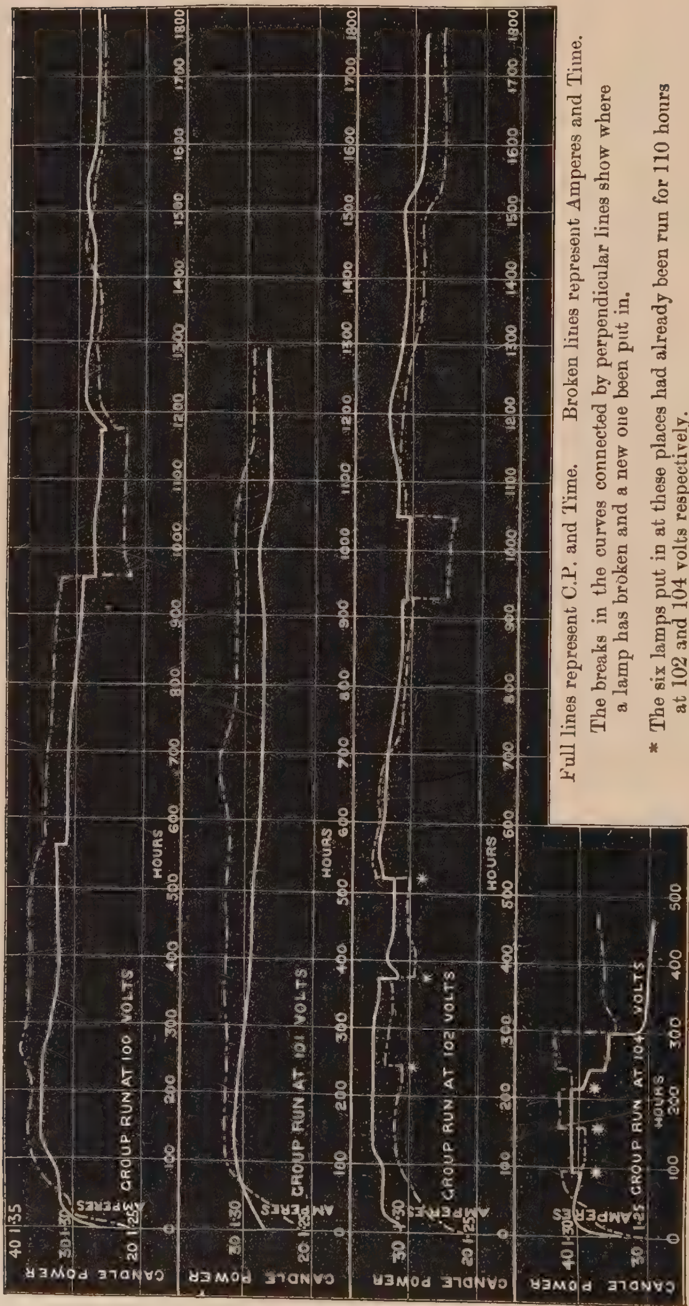
The results obtained in these preliminary tests are shown in fig. 10*a*, the curves in fig. 10 being those obtained from the lamps subsequently tested.

These curves show the variation of light and current with time for four groups of lamps between the terminals of which perfectly constant P.Ds. of 100, 101, 102, and 104 volts were maintained respectively, the full lines representing candle-power and the dotted lines current. The breaks in the curves connected by vertical lines indicate the times at which a lamp broke and was replaced by a new one, so that each group always consisted of three lamps.

All these curves show that the light given out by these Edison-Swan lamps was *greater* after they had been glowing for some time than it was when the lamps were new; also that even just before the filament of one lamp in a group broke, the total light given out by the group of three was greater than when the lamps were new. This is a totally different result from that obtained in earlier tests, as illustrated in figs. 1, 2, 3, and 4, which showed that a considerable deterioration in candle-power always took place after lamps had been running for some time. Further, while the globes of earlier lamps were always much blackened, even after a run of a few hundred hours, and so became comparatively useless long before the filament broke, the Edison-Swan lamps which we have been testing showed hardly any blackening, even when the filaments lasted for over 1300 hours.

The highness of the average candle-power of Edison-Swan

Fig. 10.—Life Tests on Edison-Swan 8 C.P. 100-volt Lamps run at Various Voltages.




Full lines represent C.P. and Time. Broken lines represent Amperes and Time.

The breaks in the curves connected by perpendicular lines show where a lamp has broken and a new one been put in.

* The six lamps put in at these places had already been run for 110 hours at 102 and 104 volts respectively.

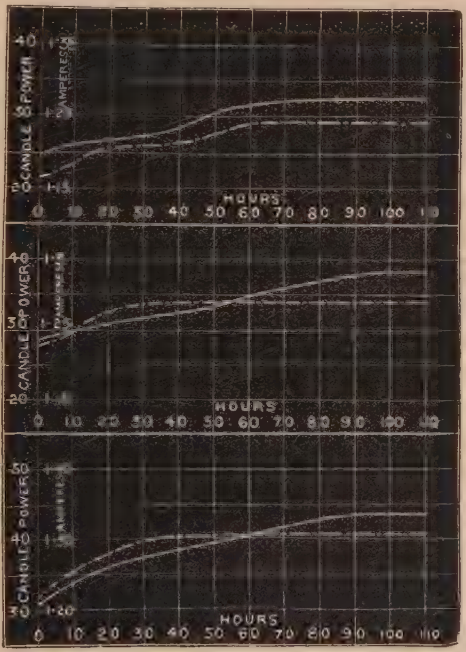
lamps marked 100–8 after they have been run for some time at 100 volts, is corroborated by the figures in the following table, which gives the results of the measurements of the candle-power and the watts per candle of eleven

Fig. 10 *a.*—Preliminary Tests on Edison-Swan 8 C.P. 100-volt Lamps run at Various Voltages.

Group of 3 Lamps marked 100–8 run at 100 volts.

Group of 3 Lamps marked 100–8 run at 102 volts.

Group of 3 Lamps marked 100–8 run at 104 volts.




Full lines represent Candle-Power and Time.
Broken lines represent Amperes and Time.

8-candle 100-volt lamps which had been run from the mains of the Notting-Hill Electric Lighting Co. As these lamps had been used in different rooms of a dwelling-house, it was impossible to say for how many hours they had been run, but the time probably varied from about 200 to 600 hours, and at any rate none of them were new.

The measurements were taken at 100 volts, and Harcourt's pentane burner was the standard of light used.

| Candle-Power. | Watts per Candle. | Candle-Power. | Watts per Candle. |
|---|---|---|---|
| 10·5 | 4·10 | 10·5 | 4·04 |
| 11·9 | 3·77 | 8·4 | 4·10 |
| 7·37 | 5·41 | 9·73 | 4·23 |
| 7·65 | 4·51 | 10·05 | 4·19 |
| 12·68 | 3·43 | 9·78 | 4·31 |
| 11·25 | 3·92 | | |

In these lamps, as in the others we tested, the blackening of the bulbs was very slight indeed. Since the above measurements were made, the two lamps whose candle-powers were 11·9 and 11·25 have broken after about another hundred hours of life without, as far as could be judged, any diminution in their light or any increase in the blackening of their bulbs.

Moreover, although the rise of candle-power seen in figs. 10 and 10 *a* was always accompanied with a rise in current, yet, as the rise in candle-power was proportionally much greater than the rise in current for the lamps we tested, as will be seen from figs. 10 and 10 *a*, where one division represents 50 per cent. change in the light, but only 4 per cent. change in the current, the consumption of power per candle was actually *less* after the lamp had run for 50 hours than it was at the beginning, and the power per candle did not rise seriously during the whole life of a lamp.

Examining the results for the group run at 100 volts, we see that in the first 124 hours the candle-power rose from 26·3 to 35, or by 33·1 per cent., the current in the same time rising from 1·261 to 1·335 amperes, or by 5·87 per cent., so that during these first hours the watts per candle dropped from 4·79 to 3·82.

During the 1820 hours of running, three lamps broke in this group, their lives being 572, 957, and 1167 hours; the

three still unbroken at the end of the tests had been run for 1248, 863, and 653 hours respectively.

Over the first 500 hours the average consumption of energy with the lamps run at 100 volts was at the rate of 4·0 watts per candle, over the second 500 hours 4·36 watts per candle, over the third 500 hours 4·55 watts per candle, and over the whole run the average efficiency was 0·23 candle per watt, corresponding with 4·35 watts per candle.

The candle-power of the three lamps run at 101 volts rose in the first 150 hours from 26·5 to 30·6, that is by 15·5 per cent., the current rose from 1·27 to 1·33, or by 4·72 per cent., the watts per candle in the same time falling from 4·80 to 4·32. The average watts per candle over the whole run of 1340 hours of this group were 4·68, corresponding with an efficiency of 0·215 candle per watt.

In the group at 102 volts which was run for 1820 hours six lamps broke, their lives being 242, 372, 516, 570, 516, and 786 hours respectively in the order of breaking; the three lamps unbroken at the end of the tests had run for 1148, 898, and 772 hours respectively.

The initial rise in candle-power in this group at 102 volts was from 30·6 to 35, or a rise in 120 hours of 14·4 per cent., the current in the same time rose from 1·26 to 1·31 amperes, or by 3·97 per cent., so that the watts per candle diminished from 4·22 to 3·82.

The average consumption of energy during the first 500 hours was at the rate of 3·9 watts per candle, during the second 500 hours 4·06 watts per candle, during the third 500 hours 4·2 watts per candle, and over the whole run the average was 4·12 watts per candle, corresponding with an efficiency of 0·243 candle per watt.

Turning to the group run at 104 volts, the curves show that the breakages were very frequent. Altogether five lamps broke, their lives being 94, 175, 210, 242, and 254 hours; the three lamps left unbroken at the end of the test had been run for 165, 222, and 364 hours.

The initial rise in candle-power was from 33·8 to 40·5 candles, that is 19·8 per cent.; and the current rose from 1·264 to 1·307 amperes, or by 3·4 per cent.

Over the whole run the average watts per candle, for the

lamps run at 104 volts, were 3·6, corresponding with an efficiency of 0·278 candle per watt.

It is known from preceding tests (see the 'Electrician,' July 15th, 1892, for example) that the light given out by a new glow-lamp varies approximately as the seventh power of the pressure when the pressure is something like the normal pressure for the lamp. Now, as the groups of lamps that we tested at 100, 102, and 104 volts respectively were selected so that each group gave practically the same light initially when tested at the same pressure of 100 volts, it was to be expected that the three groups run at the three different pressures would follow the law for the light given out by the same lamp when used at different pressures.

And this is practically the case, for if $L_{100}$, $L_{102}$, and $L_{104}$ be the number of candles emitted by the three groups of three lamps each, we see from the curves on fig. 10 that at the start

$$L_{100}=26{\cdot}3,\ L_{102}=30{\cdot}6,\ L_{104}=32{\cdot}5\ ;$$

$$\therefore\ \frac{L_{102}}{L_{100}}=1{\cdot}16,\text{ and }\frac{L_{104}}{L_{100}}=1{\cdot}24.$$

Hence the light is roughly as the seventh power of the pressure at the start.

But after the lamps have been glowing for 100 hours this relationship no longer holds, for after 100 hours from the start we see from the curves on fig. 10 that

$$L_{100}=33{\cdot}9,\ L_{102}=34{\cdot}1,\ L_{104}=39{\cdot}8,$$

or

$$\frac{L_{102}}{L_{100}}=1,\quad \frac{L_{104}}{L_{100}}=1{\cdot}17,$$

so that the light varies only as something like the fourth power of the pressure.

On the other hand, if we consider the results of the preliminary tests recorded in fig. 10$a$, we have at the start

$$L_{100}=24{\cdot}8,\quad L_{102}=28{\cdot}7,\quad L_{104}=30,$$

so that

$$\frac{L_{102}}{L_{100}}=1{\cdot}16,\quad \frac{L_{104}}{L_{100}}=1{\cdot}21\ ;$$

while after 100 hours' run

$$L_{100}=33{\cdot}1, \quad L_{102}=38, \quad L_{104}=43{\cdot}3\,;$$

so that

$$\frac{L_{102}}{L_{100}}=1{\cdot}15, \quad \frac{L_{104}}{L_{100}}=1{\cdot}31.$$

In this case, then, the law of the seventh power holds roughly not only for the light given out by the three groups at the start but also at the end of the first 100 hours' run.

The following table gives the analysis of the chief results shown by the curves on figs. 10 and 10 *a*.

| | Preliminary Tests. | | | Later Tests. | | | |
|---|---|---|---|---|---|---|---|
| Pressure in Volts maintained between Lamp Terminals | 100 | 102 | 104 | 100 | 101 | 102 | 104 |
| Duration of Test, in Hours | 110 | 110 | 110 | 1,820 | 1,340 | 1,820 | 460 |
| Initial Candle-Power per Lamp | 8·23 | 9·57 | 10 | 8·77 | 8·83 | 10·2 | 11·27 |
| Initial Watts per Candle | 4·72 | 4·37 | 4·20 | 4·79 | 4·80 | 4·22 | 3·89 |
| Largest Candle-Power per Lamp | 11·1 | 12·6 | 14·5 | 11·67 | 10·23 | 11·67 | 13·5 |
| Percentage Rise in Candle-Power at the beginning | 35·0 | 31·9 | 45·0 | 33·1 | 15·5 | 14·4 | 19·8 |
| Percentage Rise in Current at the beginning | 7·55 | 4·87 | 7·43 | 5·87 | 4·28 | 3·97 | 3·4 |
| Smallest Watts per Candle | 3·74 | 3·48 | 3·12 | 3·82 | 4·3 | 3·82 | 3·33 |
| Average Candle-Power per Lamp during whole run | ... | ... | ... | 10·0 | 9·4 | 10·7 | 12·1 |
| Average Watts per Candle during whole run | ... | ... | ... | 4·35 | 4·68 | 4·12 | 3·6 |
| Lives in Hours of Lamps Broken during run | ... | ... | ... | 572; 957; 1167. | No Lamps broken. | 242; 372; 516; 540; 516; 786. | 94; 175; 210; 242; 254. |
| Lives in Hours of Unbroken Lamps | ... | ... | ... | 1248; 863; 653. | 1340; 1340; 1340. | 1148; 898; 772. | 165; 222; 364. |
| Number of Lamps included in Striking Averages | ... | ... | ... | 6 | 3 | 9 | 8 |

Although, as already stated, the number of lamps that could be supplied with power every night at the Central Technical College was limited to nine, and although, therefore, the total number dealt with in the investigation was small, the curves on figs. 10 and 10 *a*, and the results given in the preceding

table, are quite sufficient to enable us to arrive at the following results :—

(1) When a group of Edison-Swan lamps marked 100–8 are run at 100 volts, and each lamp, as its filament breaks, is replaced by a *new* Edison-Swan 100–8 lamp, it may be expected that the light given out by the group will never be as small as it was at the beginning, when all the lamps in the group were new. This important result is brought about by the deterioration of the lamps with long-lived filaments being compensated by the great rise in the light given out by each new lamp when put in place of one whose filament has broken.

(2) An Edison-Swan lamp marked 100–8 when run at 100 volts will give an average illumination during its whole life of about 10 candles, and will absorb an average power of about 4·3 watts per candle, so that such a lamp must be regarded as a 43-watt lamp, and not a 30-watt lamp as is not unfrequently stated, this difference in power being about 43 per cent.

(3) An Edison-Swan lamp marked 100–8 may, when run at 100 volts, emit during a large portion of its life a light of as much as 11·7 candles, and absorb a power of about 44·6 watts, which is about 44 per cent. greater than the nominal 30 watts.

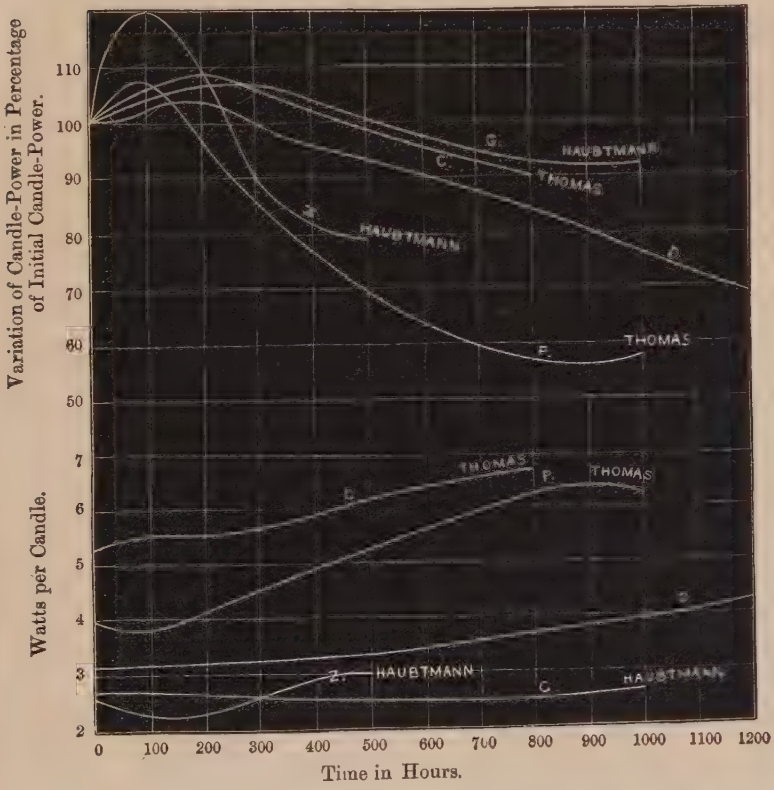
These last two facts are of great importance to dynamo-constructors, when specifying, as is frequently done, the number of lamps of a given type to which a given dynamo can supply current to without becoming too hot. For if, in making such a calculation, it be assumed that an Edison-Swan 8-candle-power lamp absorbs the nominal 30 watts, that is 3·75 watts a candle instead of the 44·6 watts which our tests show that such a lamp actually absorbs during a long portion of its life, the current will be 44 per cent. greater, and the rate of heating of the dynamo 108 per cent. greater, than was anticipated.

(4) Groups of new Edison-Swan lamps marked 100–8, if selected so as to give the same light at 100 volts, will, *when new*, emit a light which is roughly proportional to the seventh power of the pressure applied to them.

But after a run of 100 hours, this rule connecting light and pressure may, or may not, hold.

The rise in candle-power with time, which occurred during the early part of the life of all the Edison-Swan lamps which we tested, may be noticed also in the collection of curves published by Mr. Feldman in the 'Electrician,' Nov. 29th, 1892 (fig. 11). But there are certain very important differences between the results recorded by these curves and those

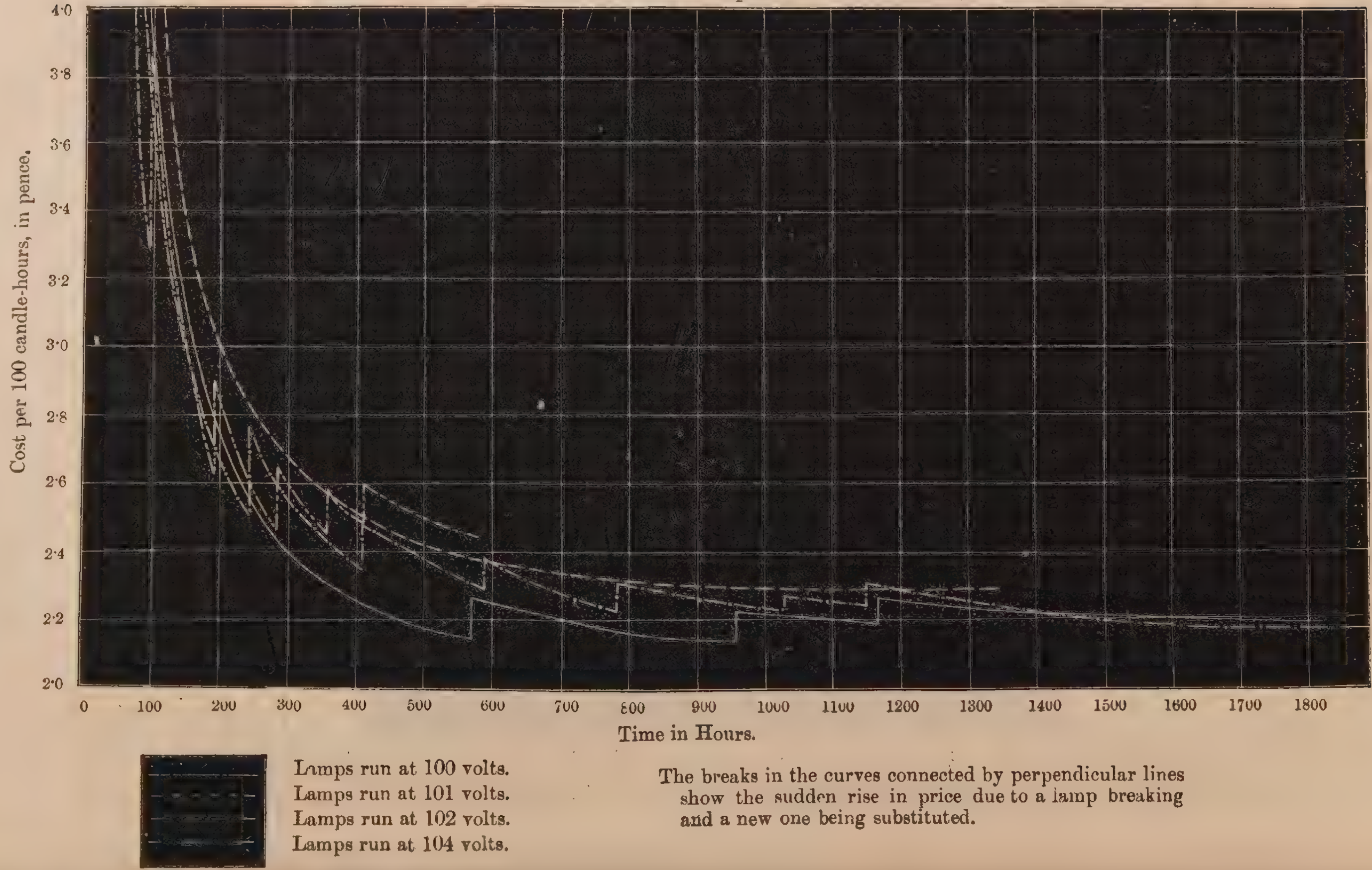
Fig. 11.




which we have obtained. These differences are shortly as follows:—

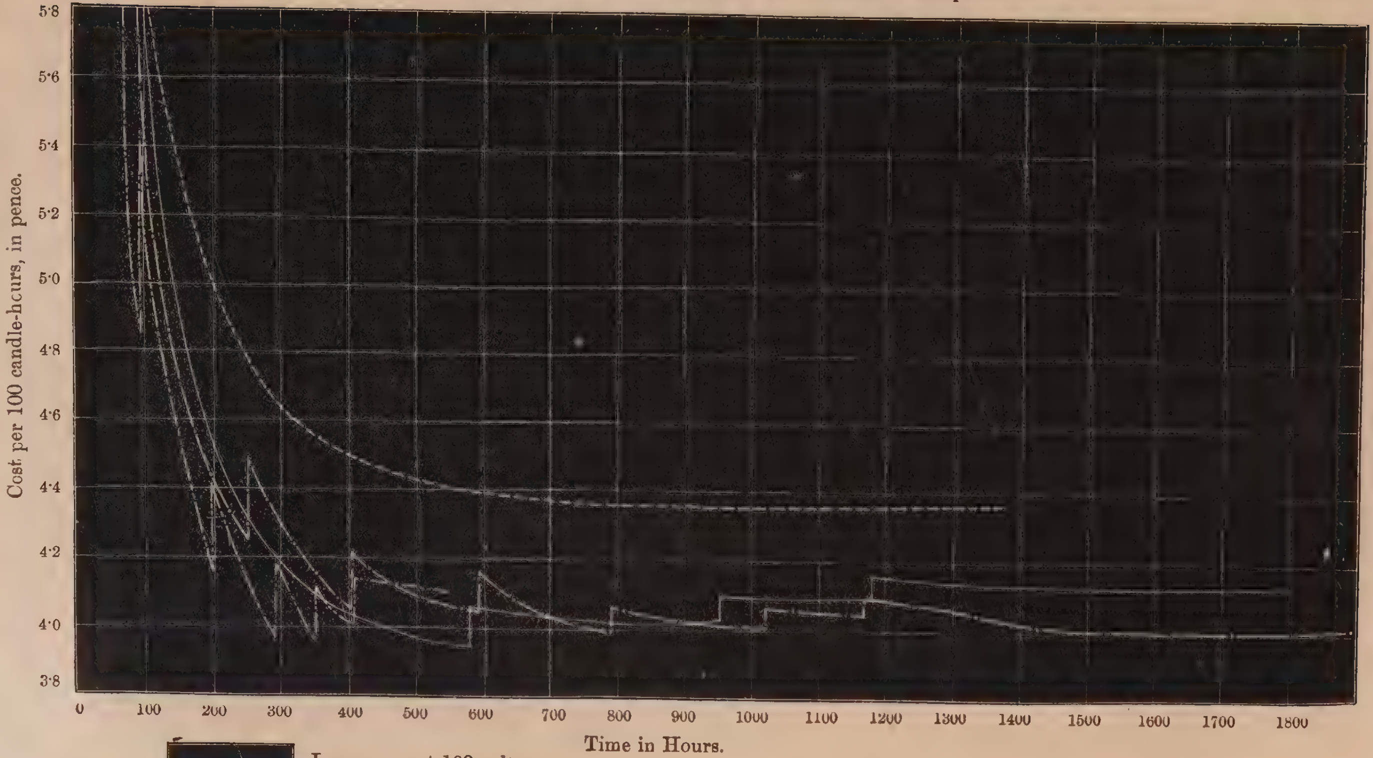
(1) The greatest rise of light emitted by the lamps as recorded in Mr. Feldman's curves was 14 per cent. In our experiments this maximum rise was 45 per cent.

Fig. 12.—Life Tests on Edison-Swan 8 C.P. 100-volt Lamps. Curves showing the relation between Cost of Light and Time. Price of a Board of Trade unit $4\frac{1}{2}d.$ Cost of a new lamp 1*s.* 9*d.*




The breaks in the curves connected by perpendicular lines show the sudden rise in price due to a lamp breaking and a new one being substituted.

Price of a Board of Trade unit 9*d*. Cost of a new lamp 1*s*. 9*d*.




Lamps run at 100 volts.
Lamps run at 101 volts.
Lamps run at 102 volts.
Lamps run at 104 volts.

The sudden breaks in the curves connected by perpendicular lines show the sudden rise in price due to a lamp breaking and a new one being substituted.

(2) In spite of the first rise in the candle-power as shown in Mr. Feldman's curves (fig. 11), the candle-power at the end of the life of the lamps was in all cases much less than it was when the lamps were new. Whereas with the lamps which we tested at 100, 101, and 102 volts the light given out by a group was never as low as it was at the beginning when the lamps were new.

(3) In spite of the first rise in candle-power recorded in the curves on fig. 11, it was only for the lamps whose behaviour was recorded in curves P and Z that the power expended per candle diminished at first with time. And even in the case of these two sets the power expended per candle increased again, and was greater when the filaments broke than it was when the lamps were new. Whereas in our tests the power expended per candle not only diminished considerably during the early life of the lamp, but it never rose again as high as it was when the lamps were new.

Although, then, the rise in candle-power during the early part of the life of a glow-lamp is apparently not an absolutely new fact, the magnitude of the rise and the effects resulting from it were, in the older lamps, so trifling that no special attention seems to have been devoted to this important subject in the former reports of tests of lamps. Indeed, even in a prominent book connected with glow-lamps which has quite recently been brought out in this country, no mention whatever is made of the fact in question. And yet, as we have already shown, and as will become more apparent from what follows, this remarkable rise in the candle-power during the earlier part of the life of an Edison-Swan lamp has a very important effect on the economy of lighting with glow-lamps.

The next point toconsider is the way in which the cost of lighting with the modern 100–8 Edison-Swan lamps depends on the pressure at which they are run, and on the cost of a Board of Trade unit. The curves on figs. 12 and 13 give the cost of obtaining light with the various groups of lamps, including the cost of replacing lamps with broken filaments, calculated day by day from our tests as the experiment went on.

The ordinates of the curves show the cost of 100 candle-hours

# PHYSICAL SOCIETY OF LONDON.

## ABSTRACTS OF PHYSICAL PAPERS

### FROM FOREIGN SOURCES.

JULY 1895.

### GENERAL PHYSICS.

484. *Andrée's projected Balloon Expedition to the Pole.* **G. Tissandier.** (C. R. 120. pp. 996 997, 1895.)—Owing to the inevitable losses of gas consequent upon variations in temperature and the density of the atmospheric strata, none of the actual balloons can make journeys of more than 24 hours' duration, whilst Andrée requires 30 days for his polar expedition. Andrée's statement that Giffard's balloon retained its charge for three months in the Carrousel in 1878 is incorrect; the hydrogen was replenished almost every day. A balloon expedition to the Pole may not be impossible, but we should need other kinds of balloons. H. B.

485. *Effects of Expired Air upon Animal Life.* **D. H. Bergey, S. W. Mitchell,** and **J. S. Billings.** (Nat. Ac. Sci. April 16, 1895; Science, May 3, 1895.)—This summary of the conclusions of a report states:—The air expired by healthy mice, sparrows, rabbits, and men contains no injurious organic matter; in ordinary respiration no bacteria, epithelial scales, etc. are ejected such as might be thrown out by coughing and sneezing; the minute quantity of nitrogenous and other oxidisable compounds in the condensed moisture of the human breath appears to be largely due to a decomposition of organic matter in the mouth and pharynx; the contamination in hospital wards arises partly from dust particles, which carry moisture, ammonia, and bacteria. Experiments made with animals compelled to breathe air vitiated by respiration, or

injected with condensed fluids from such, do not confirm the view of Hammond, Brown-Séquard, and Merkel, that such air contains volatile poison, although no conclusions as to human beings can be derived from the fact that a mouse can live for months even in air vitiated essentially by carbonic acid. An artificial atmosphere, rich in carbonic acid, poor in oxygen, seems to have the same effect as expired air. That animals can accustom themselves to living in very bad atmospheres has been observed in exceptional cases only. Excessively high or low temperatures have a decided effect upon asphyxia, by increase of carbonic acid and diminution of oxygen; at high temperatures the evaporation from the skin and mucous surfaces is checked by saturation of the air; at low temperatures the demand for oxygen increases. The changes in the proportions of the two gases are, however, not sufficient to account for the discomfort experienced in badly ventilated schools, barracks, etc.; nor can they explain the high death-rates of the British Army—foot-guards 29·4, infantry 17·9, against 11·9 civilians. Bad ventilation tends to produce tubercular disease, unless the persons are of the Jewish race. As the specific bacteria of pneumonia and consumption adhere to dust particles, particular rooms may be more liable to infection; but we have no proofs, as yet, that the deleterious effects are exclusively due to an increase in pathogenic micro-organisms. The discomforts of over-crowded rooms are chiefly to be ascribed to excessive temperature (overheating by the illuminants) and unpleasant odours; the musty atmosphere has not sufficiently been explained, but may partly be due to decayed teeth, disordered digestion, etc. The report emphasizes the necessity of preventing dust, of regulating temperature and moisture, of excluding the products of combustion, and also of further study, since the foundations of modern systems of ventilation are doubtful. H. B.

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486. *Density of Helium.* **Clève.** (C. R. 120. p. 1212, 1895.)—A letter from Clève to Berthelot, stating that the density of helium has recently been determined by Langlet. The gas, extracted from clèveite, was passed over heated copper oxide to free it from hydrogen, and over metallic magnesium to get rid of nitrogen. It contained no argon. The density was found to be 0·139 (air = 1) or 2·02 (hydrogen = 1). This is much lower than the density found by Ramsay. D. E. J.

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487. *Abnormal Variations of Pressure with Height.* **L. Teisserenc de Bort.** (C. R. 120. pp. 846–849, 1895.)—On measuring the difference of pressure between two points situated on the same vertical, and comparing it with that calculated from the density of the air and the intensity of gravitation at the two points, a divergence is found between the two numbers. This divergence, referred to the distance between the two points, constitutes a true gradient, which the author calls the *vertical*

*barometric gradient.* Since the experimental demonstration of this gradient at the Puy-de-Dôme in 1884, the barographs at the Eiffel Tower have made its study more convenient. It appears that the static law of the decrease of pressure with the height is rarely satisfied. The gradient is negative when the calculated is less than the observed pressure, and the resultant is directed upwards, and *vice versâ.* The gradient presents a well-marked diurnal variation, negative gradients increasing towards noon. The variations are greater in summer than in winter. Thus, at a height of 279·5 m. on the Eiffel Tower, the mean of three years in January was −0·15 mm. at midnight and −0·21 mm. at noon, and in July +0·07 mm. at midnight and −0·17 mm. at noon. E. E. F.

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488. *New Method of Measuring the Freezing-Points of very Dilute Solutions.* **A. Leduc.** (J. Phys. 4. pp. 162–168, April 1895.) —The author proposes that, instead of observing the freezing-points directly by means of a thermometer, the excess of pressure necessary to keep the solution in equilibrium with ice at 0° C. should be determined. The pressure can be produced by a column of mercury, and an error of 1 mm. in reading the height of this column would correspond to an error of freezing-point of only one hundred-thousandth of a degree. The connection of this effect with the osmotic pressure is then worked out, and Van't Hoff's formula for the depression of the freezing-point is deduced directly by considering the equilibrium of a column of the solution at its freezing-point, contained in a tube, with a mass of pure water at the same temperature, which is frozen to a depth at which the pressure is great enough to keep it liquid. W. C. D. W.

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489. *Thermal Conductivity of Copper.* **R. W. Quick, C. D. Child,** and **B. S. Lanphear.** (Phys. Rev. 2. pp. 412–423, 1895.)—Prismatic bars of electrolytic copper were used, one end of which was kept at any desired temperature by means of a solenoid of iron wire heated by a current from an accumulator. The temperatures along the bars were measured by the change of resistance undergone by a small closely-fitting coil of copper wire, the resistance being measured by a slide-metre bridge. The temperatures along the bar when equilibrium was attained were plotted in a "statical curve," and the temperatures and times of cooling of the bar as a whole were plotted in a "time-curve." From these two curves the "integration curve" was constructed, and from this the conductivity was calculated by the ordinary formula, allowance being made for changes of specific heat and of density. The conductivity thus determined in C.G.S. units was 0·914 at 74°, and increased up to 1·024 at 166°·8 C. E. E. F.

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490. *Colour Relations of Atoms, Ions, and Molecules.* **M. C. Lea.** (Am. J. Sc. 49. pp. 357–374, 1895.)—The colours here

considered are not those of the elements, which are of little significance, but of the atoms. Between the two there exists no relation. The colour of an atom can never be deduced from that of an element. But in any coloured inorganic compound in solution, the colour belongs essentially to the metallic atom; and the colour of the ion and that of the atom are essentially the same. The value of the author's conclusions must depend upon the validity of his "criteria" for the colour of ions, among which may be mentioned :—(1) "When an electrolyte gives a solution in water which is colourless when dilute, both the kathion and anion are colourless. . . . Sodium monosulphide gives a colourless solution in water; therefore the atoms of sodium and of sulphur are colourless." (2) "All elementary anions are colourless." (3) "Many composite anions are colourless, for example $SO_4$. So that when sulphates give solutions that are coloured when dilute, the colour must be due to the kathion." The author next proceeds to give, "first, a new classification of the elements based on more correct principles than those previously made; and, second, a proof that the colour or non-colour of an element is a function of its atomic weight." One metal, zirconium, has proved rebellious to his classification: there are a number of blanks in his table, and he is doubtful whether all these represent so many elements remaining to be discovered. But he arrives at the result "that we must reject the well-known Periodic Law as being based upon erroneous principles." D. E. J.

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491. *The Geology of the Moon.* **E. Suess.** (Wien. Ber. 104. Part I. pp. 21–54, 1895.)—With the help of the enlarged reproductions, made by Weinek of Prague and Prinz of Brussels, of the moon photographs taken at the Lick Observatory and by Loewy and Puiseux at the Paris Observatory, Suess reviews our knowledge of the geology of the moon, assuming that we have there to deal essentially with rocks and minerals like our own. There are important differences in the forces. The gravity on the moon is only one-sixth of that on the earth; the effect of gravitation on magmatic differentiation can, therefore, only be slight. The mean density of the moon being 3·4, lighter, acid rocks are more likely to occur on its surface than a lava as heavy as that from Hawaii, sp. gr. 3·3. The moon has no atmosphere which would influence fusion and evaporation; whether there has ever been an atmosphere, the author does not discuss. The great differences in day and night temperature must lead to powerful disintegration, and must have hastened solidification on the dark hemisphere. On the whole, the author agrees with the opinions which Dana expressed fifty years ago.

Moon scenery has been compared to the panorama of Naples. In this panorama we should, at full earth, perceive no relief, no craters, but the alum of the solfatara would shine out brightly from the crater-edge bleached by acid vapours. Schröter and, afterwards, Neison have distinguished ten degrees of illumination for the moon :

0° is the darkest shadow, 2° to 3° visible in some of the maria, 3° to 4° on the inner slopes, 4° to 6° the yellow-grey of most mountains, 7° and above rare on the whole, 10° the maximum intensity almost confined to the interior of Aristarchus. The heights appear lighter than the plains ; the brightest spots, evidently of more recent origin, are scattered and have distinct outlines. A similar scale has been applied to the earth, where the darkest shades, 1° and 2°, would be represented by various basalts, greatly differing in composition ; andesites and trachytes would give the mean greys ; and white rhyolites, fumaroles, white ashes, and pumice-stone, that is to say also the newest formations of limited area, would supply the brightest tones. Landerer found the polarising angle of the grey surfaces of the Mare Nectaris and others to be 33° 17′ ; telluric basalts gave 31° 43′, trachyte 32° 16′. andesite 32° 50′ ; a vitrophyr from Rhodope with large crystals of sanidin, magnetite, hornblende in a fluidal, not perlitic mass, 33° 18′ ; hyalomelan 33° 39′ ; obsidian 33° 46′.—White may be due to various causes. Vulcano (Lipari Islands) had been sending up fumaroles, but had otherwise kept quiet, since 1771, when in 1873 snowy siliceous ashes were ejected for three hours ; subsequent eruptions threw out grey ashes and blocks of lava bleached by acid vapours. Simony's photograph of the Pico de Teyde shows a white spot due to fumaroles. The solfataras of a long fissure in the Chilian Andes and the trachyte blocks appeared white to Domeyro when he visited the scene three months after the formation of the fissure in 1846 ; in 1857 he found the surface grey, in 1873 black ; the solfataras had ceased. Such white fissures are common on the moon ; in Aristarchus they radiate from the intensely, but not uniformly bright centre, 35 km. in width. A spot in Werner is said to have lost its brightness. The striking bright rays starting from Tycho, Copernicus, etc. resemble white shadows. They have no relief, cannot be fissures, as the crater-walls seem intact, nor ejected pumice, as they extend much too far : in some instances they appear curved. They might mark ancient fumaroles from finer clefts, which, like the Chilian fissure, may have formed near apparently extinct volcanoes. Their great length is remarkable ; but the basaltic Cleveland dyke in Scotland reaches, according to Geikie, a length of 174 or even 300 km. According to Fouqué, five phases may be distinguished in fumaroles. From the fused lava of 500° and more, anhydrous fumaroles, rich in chlorine, escape ; the lava becomes incrusted with sodium chloride. Then follow, at temperatures between 300° and 400°, hydrochloric acid, sulphurous acid, and large quantities of water vapour : these acid vapours are replaced at 100° by alkaline ejections, water, and sulphuretted hydrogen ; on further cooling, carbonic acid begins to prevail, the final mofetti consist essentially of carbonic acid. On the moon we should have to assume such phenomena on an extraordinarily large scale. But if we accept temperature differences in the diurnal range anything like those suggested by Rosse, those phases may have been subjected to

periodical variations of long duration, and the water might be accounted for in other ways. The water, which undoubtedly plays a most important part in our volcanic phenomena, need not have been supplied by infiltration of sea-water, but may have been absorbed by the fused masses. This is the theory of Angelot, which Tschermak applied to the moon. Fused metals and glasses have an extraordinary power of absorbing gases. In casting steel ingots, the mould has repeatedly to be filled again: the "pipe" which forms is produced by the escape of gases. If a cover is put on for a few minutes, one or more eruption-cones may be seen; the pipe afterwards shows ledges of steel. Dana observed the same phenomena—rents in the crust, refusion, black ledges—in the Kilauea, where the replenishing ensues from below. F. C. G. Müller ascribes the rise of the fluid, resulting in a lifting of the crust, which afterwards contains capillary tubes charged with hydrogen and also nitrogen, to these gases; the final foaming and spitting seems to be caused by bubbles of carbonic oxide rising vertically in the core of the ingot. The appearance of the moon is that of a cooled, fiery mass; we see no traces of water, of aqueous sediments, of folded strata, nothing but circular or perhaps elliptical contours. During the cooling shoals of slag must have formed, they have been refused, and fluid basins have been produced, pushing their scum and slags, like moraines, to the outside, where they heaped up to walls thousands of feet high. These walls are the so-called mountain ridges, the Alps and Apennines. This view would explain the union of such rings (Imbrium and Sinus Iridum). The lunar Alps are crossed by a valley 130 km. long, flat-bottomed, with vertical walls more than 3000 m. in height; the valley starts with a width of 9 km. from the Mare Imbrium, becomes narrower, and shows near its end on the Mare Frigoris a Z-shaped displacement reminding one of the two laterally displaced parts of a cracked sheet of ice.

Although most lunar waters are younger than the respective mare, hills like Archimedes and Copernicus cannot be ejection cones. The bottom of these water-mounts lies always far (in 47 out of 92 cases from 1000 to 2000 mm.) below the level of the mare, so that these craters reach depths of 4612 m. (Tycho) and 4630 m. (Simpelius). In those waters the lava may, as in the steel ingot, repeatedly have risen and overflown. Some of them—Catharina, Cyrillus, and Theophilus—are close to one another and encroach upon one another. From what has been said, it might be thought that refusion should always occur in the same chimney. But a study of the isogeothermal lines shows that strong crusts are formed at the bottom of such hollows, and that weak spots are left not always in the bottom, but by the side of the forge. Most of the Mare, Palus, and Lacus would then be hearths of refusion, and their craters would correspond to the Hawaii type, from which they differ by their greater radius and inner depression below the outer level. Different from these craters are the cup-shaped hollows in regular cones, with regular walls, sharp edges, and

undistinguishable bottoms, looking as if enormous bubbles had burst from the lava, and the interior caved in afterwards. They are smaller, diameters from 250 m. (the minimum as yet determined) to 15 and 18 km., occur in groups, sometimes 8-shaped, and apparently without separating walls, all over the moon, and may have been produced by single gas explosions or rather evolutions which, as Humboldt and Dana pointed out, might acquire vast dimensions on the moon. Many of these minor craters are undoubtedly connected with fissures, and may correspond to the adventive craters of Etna, which impart, at sunrise, to Etna a moon-like appearance. During September 1783 a fissure formed west of the Skaptar Jökull (Iceland), crossing the whole island in a south-westerly direction, and cutting the volcanic mountain Laki. Helland found, in a length of 20 km. of this fissure, 30 crater cones, not higher than 150 m., heaped up of ashes and slag and discharging lava like the fissure itself. Thoroddsen counted later about a hundred cones. The newer moon photographs show many such furrows; whether they contain cones or only funnels, and whether they are not, in some spots, bridged by ridges without cutting them, remains undecided. The cause of the different appearance of the bright shadows of Tycho etc. when photographed or viewed through the telescope, is still unexplained. The author emphasizes that we need photographs of the same parts at different illuminations, and terrestrial photographs under strong illumination for comparison, and makes other suggestions. H. B.

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492. *Daily Movement of Wind Velocities in the United States.* **F. Waldo.** (Am. J. Sc. 49. pp. 431–442, June 1895.)—In Appendix 14 to the Chief Signal Officer's Annual Report for 1890, the average wind movements are given for a large number of stations in miles per hour for the 24 hours of the day, for each month, and also the averages for all the months of the seven years 1883–89. That most valuable tabular compilation arranges the velocities according to synchronous hours of the 75th meridian time. The author has tabulated the data for the four midseasonal months and for the year, according to local times, and has grouped the stations geographically instead of alphabetically. The present paper, a digest of the official report, discusses the daily march of the hourly wind velocities for January, July, and the year, for 20 stations—on the Atlantic Coast, Mexican Gulf, Great Lakes, Pacific Coast; Inland, North-east, South-east, Lower Mississippi, Ohio River, Lake Region, Upper Mississippi, Great Plains, Great Plateau. Twenty curves are given for each January and July. General conclusions are not drawn. On the whole, the July curves contain the maxima between $12^h$ and $16^h$; and whilst the average wind velocity is on the exposed Atlantic coast greater during January (averages 16·2 against 10·9 miles), the steepest daily gradients (San Francisco 6·7 to 17·9 miles) occur during the summer months. This applies also to the Plateau, where the

January extremes are 2·5 and 4·6, and the July extremes 1·3 and 8·5 miles per hour. Tatoosh Isl., North Pacific, has in January a principal minimum at midday, but the velocity varies only between 15·3 and 17·0 miles; in July the velocity is much less, the maxima and minima are shifted, but the gradients are hardly steeper than during the winter. This is a striking contrast to San Francisco on the Central Pacific, where the January curve does not rise much above 5. H. B.

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493. *Rise in Solubility.* **H. Goldschmidt.** (Zschr. phys. Chem. 17. pp. 145–163, 1895.)—According to Nernst's theory of solubility the addition of a small quantity of a non-electrolyte to a solution of a salt, which crystallises without water of crystallisation, has no, or at most only a slight effect on the solubility of the salt. It is otherwise with a salt which crystallises with water of crystallisation: the addition of a non-electrolyte raises the solubility, and molecular quantities of different non-electrolytes produce equal rises. Taking the transition temperature of a hydrated salt, it can be shown theoretically that this temperature will be depressed like the freezing-point of a solvent, and that at any temperature the solubility of the hydrated salt will be increased. Experiments are made with sodium paranitrophenolate, which crystallises with $4H_2O$ and with $2H_2O$. The transition temperature at which the tetrahydrate becomes the dihydrate, found by a dilatometer, is 36°; determined by the solubility curves, it is 35°·79. Molecular quantities of the following non-electrolytes—urea, glycerine, acetone, propionitril, acetonitril—produce practically the same increase in solubility, whilst ethyl-alcohol produces hardly any change, *i. e.* it does not act like a foreign body. The depression of the transition temperature for glycerine is 1°·91, and for acetone 1°·67. This is a good agreement with the theory. Van't Hoff's equation

$$\frac{d \log c}{dT} = \frac{Q}{2iT^2},$$

where $c$ is the solubility, enables the author to calculate the ratio of $\frac{Q}{i}$ for the tetrahydrate and $\frac{Q'}{i}$ for the dihydrate, Q and Q′ being the respective heats of solution. Then, with the aid of the calorimetrically determined heat of hydration $W_H$, it is possible to calculate $i$ the ionisation constant from the equation

$$W_H = i\left(\frac{Q}{i} - \frac{Q'}{i}\right).$$

This gives $i$ equal to 1·55. A special determination from the electrical conductivities gave 1·6 for $i$ at the transition temperature, a value not largely different. S. S.

494. *Automatic Recording Instrument for End Measurements.* **L. Hartmann.** (C. R. 120. pp. 1024–1029, 1895.)—By the aid of linear standards with marks we can measure to 0·001 mm. Hartmann and Mengin have constructed an instrument (briefly described, not illustrated) which measures, with the same accuracy, any length by comparison with end standards. On a bench, the rulers, standards or test-pieces are held between two mandrels terminating in pistons. One mandrel is moveable; the other fixed; one ends in a screw to the circumference of which a constant weight is applied. The screw is turned in either direction by a dynamo provided with a reduction gearing. Clock-work turns a drum on which needles fixed to the screw mark points. The standard and the test-pieces are taken up alternately. The screw first recedes: during this time a new test-piece is introduced. The screw then advances until it makes contact, which is recorded on the paper. In this way a point is marked every minute, and two curves are obtained, one for the standard and one for the test-piece, which will be parallel though not regular at all temperatures, provided the two rulers be of the same material. The arrangement is such that a difference in length of 0·002 mm. would be marked by a distance of 2 mm. The standards are hollow cylinders 12 mm. in diameter, which are very stiff, light, and easily influenced by temperature changes. Their ends are spherical surfaces fitting over the pistons. They are placed in nickeled brass tubes. The screws have a pitch of 0·001 mm. 27 standards make a complete set for one machine. To measure a length of 352·25 mm., the respective ruler and a standard of 150 mm. would be placed in one tube, and two standards of 400 mm. and 102 mm. in the other. The instrument is very convenient also for determining the diameters of cylinders and for testing their curvature; further, for determining temperature coefficients. Thus a steel containing 25 per cent. of nickel gave an expansion coefficient of 0·000018, whilst the steel gave 0·0000107. Any experimental errors are at once indicated by the irregularity of the curves. Cornu testified to the remarkable accuracy obtained. The pressure being always the same, test-pieces of the same material will be equally affected, but those of different materials will not be. This explains the apparent experimental errors of some comparisons made by other means with platinum standards. H. B.

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495. *Permeability of Solid Bodies to the Ether.* **L. Zehnder.** (Wied. Ann. 55. pp. 65–81, 1895.)—With regard to the question whether the movement of a body affects the velocity of the light passing through it, Fijeau and Fresnel made three hypotheses:—I. The ether adheres to the molecules and shares their movement; II. The ether is independent and does not take part in the movement; III. Part of the ether shares the movement. Assuming the luminous ether everywhere of equal elasticity, but not of

equal density, Fijeau later modified the last hypothesis: III. $\alpha$. So much of the ether travels with the body as corresponds to the excess of its density over the density of the ether without. Fijeau's experiments with currents of water would be in agreement with III. $\alpha$, if we take the velocity of the central portions of the current, not like Fijeau the mean velocity of the water (Michelson and Morley). The author experimented with highly rarefied cylinder with an iron piston and a post at each extremity communicating with a long U-tube. If we assume that practically the whole volume is filled by the ether, neglecting the mass of air-particles, the ether would, on working the piston up and down, according to I. be driven through the U-tube like a gas, as there can be no equalisation of pressure through the piston; according to II. the ether of the piston would remain behind on the upstroke, and the ether would circulate through the U in the opposite direction; according to III. $\alpha$ the ether would neither be condensed nor rarefied, and no movement would ensue. The last two cases become doubtful if the refraction indices of metals (silver, gold, copper) are less than 1, as Kundt has asserted. If the air particles occupy an appreciable volume, being themselves impenetrable to the ether, they will push a certain volume of ether before them, so that a slight movement of the ether would result in III $\alpha$. The experiments failed, however, to show any shifting of the interference-bands, although a displacement by $\frac{1}{40}$ of the width of a band could have been detected. Hypothesis I. would thus be disproved; if $\epsilon$, the ratio of the volume of air-particles to the total volume, is great, the experiments would favour II.; if $\epsilon$ is small, III. $\alpha$. If we assume III. $\alpha$ as correct, $\epsilon$ would be 1/560. In any case the experiments establish that solids, *e.g.* iron, are extraordinarily permeable to the ether. Neumann's theory, that the ether has in all substances the same density but not the same elasticity, would not affect the results. The author inclines to the belief that both density and elasticity vary. The apparatus was made by Klingelfuss of Basel. The cast-iron cylinder had a length of 33·2 cm., the piston 10 cm. diameter and 20 cm. stroke; the U-tubes of brass had a length of $2 \times 146$ cm., and a width of 0·6 cm. Leakage was, successfully so far as tests prove, prevented by applying grease and an alcoholic solution of shellac and mastic to which oil of turpentine was added. The piston was repeatedly coated with this varnish; it further contained a ring-shaped cavity filled with mercury. The piston speed rose to 150 cm./sec. The cylinder was very firmly secured; and the author is satisfied that all possible precautions had been taken. To investigate the further problem, whether any relative movement between the earth and the ether of the universe could be detected, the author took two brass tubes, 50 cm. long, with a funnel at one end, forked at the other; the one, straight part was closed, the T-branch could be closed by mercury rising in the stem of the T. The apparatus was tried placed west-east in the plain and on the isolated Rosskopf near Freiburg. A shifting by about 80 interference-bands

might have resulted, but nothing was discovered. The author admits that experiments of this type remain ambiguous. H. B.

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496. *Relation between Relief and Seismological Frequency.* **De Montessus.** (C. R. 120. pp. 1183–1186, 1895.)—The author recurs to the classification of seismological data, historical, periodical seismologic observations, and seismographic records (these Abstracts, p. 212), but he employs different symbols, and expresses their interrelation by a different equation:

$$\log S_{i.g} = \log S_h - 0{\cdot}3080834 = \log S_{gh} + 0{\cdot}4043049.$$

By comparing unstable districts with one another, he arrives at the following laws. In a group of adjacent districts, those with the greatest slopes present the greatest instability, and the unstable districts lie along the corrugations, emerged or immerged, of the earth-crust. These laws are relative only, as the general seismicity depends upon the nature of the ground and other conditions. Mountainous districts are less stable than plains; the centre of disturbance generally coincides with marked changes in the slope, where we may assume a line of least resistance. The short and steep side of a chain is the more unstable, and particularly so in its steepest parts; the central regions of a valley are, however, less unstable than the higher and lower regions; the exterior angle under which two chains meet is less stable than the interior angle. In these latter laws exceptions are more frequent, "since we pass from geographical relief to topographical accidents." Shores of seas rapidly increasing in depth, especially along mountain-ridges, are more unstable than gently sloping shores: the centres of disturbance need not be on shore (Peru, Chili, Japan, Southern Atlantic). In some countries these general laws appear contradictory. Thus in New Zealand the unstable Eastern slope is gentler but bordered by a sea of 2000 m. in depth, whilst the Western Sea near the high mountains is comparatively shallow. All these laws only hold good for larger districts, not for isolated centres of disturbance. Although there are frequently disturbed districts which are also volcanic, those two classes of phenomena seem to be independent of one another. This part requires further study; in general, however, earthquakes must be regarded as geological phenomena, and as the last manifestations of the dynamic forces which have shaped the surface of our earth. H. B.

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497. *Critical Temperature of Hydrogen.* **L. Natanson.** (Bull. Acad. Sci. Cracowa, pp. 93–99, March 1895.)—The critical temperature of hydrogen has never been determined experimentally, although Olszewski had given reasons for concluding that this temperature is below $-220°$ C., and has shown that the critical pressure is 20 atm. Natanson now deduces the values of the former from theoretical considerations, one of these being the so-

called law of correspondence, according to which the critical temperature, pressure, volume, and molecular weight of a gas are connected by the formula

$$t_c = \mathrm{AM}p_c v_c,$$

where the value of A is the same for all gases; and the other being Van der Waals' well-known isothermal equation

$$(p+a/v^2)(v-b)=\mathrm{R}t,$$

which gives

$$v_c=3b, \quad t_c/p_c=8b/\mathrm{R}.$$

For hydrogen $a$ is too small to be determined experimentally, but from Amagat's experiments on the compressibility of hydrogen, $b$ can be determined; and taking $b=0{\cdot}00070$, Natanson infers that the critical volume of hydrogen is 23·45 cm.$^3$/gm., and its critical density is therefore 0·043 gm./cm.$^3$, whence assuming with Olszewski that the critical pressure is 20 atm., it follows that the critical temperature of hydrogen is about $-232°$ C., or 41°·3 absolute.

A second determination of the critical temperature is based on Olszewski's experiments on the adiabatic expansion of hydrogen, and this leads to the result

$$t_c = 41°{\cdot}7 \text{ absolute, or } -231° \text{ C. approximately.}$$

To form an estimate of the probable accuracy of the method, Natanson tests it for the case of oxygen, and he concludes that the critical temperature of hydrogen may really be a few degrees above the theoretical value $-231°$ C., say, for example, about $-228°$ C.

G. H. B.

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498. *Freezing of Solutions at Constant Temperatures.* **A. Colson.** (C. R. 120. pp. 991–993, 1895.)—The presence of foreign matter in solution in a liquid is known to lower the temperature at which the liquid freezes, under constant pressure. Instead of keeping the pressure constant, M. Colson keeps the temperature constant, and finds the pressure necessary to cause the solvent to solidify at that *fixed temperature.* The fluid used was benzene, and the experiments were made with solutions containing about 2½ per cent. of benzoic acid, acetic acid, naphthalene, paradichlorobenzene, and metabinitrobenzene. The table of results thus obtained shows that the pressure necessary to compensate the lowering of the melting-point has no relation to the molecular weight of the substance, since naphthalene has double the molecular weight of acetic acid, yet solutions of the same strength require nearly the same compensating pressure. On the other hand, taking three solutions in which the melting-point is lowered by nearly 1°, it is found that the pressure required to compensate a lowering of 1° is nearly the same for each, being 194 for chlorobenzene, 206 for naphthalene, and 200 for acetic acid. In the case of a 5 per cent.

solution of chlorobenzene, the lowering of the melting-point was 1°·85, and the compensating pressure 410 units, the ratio of these numbers being 221. The compensating pressure is thus approximately proportional to the lowering of the melting-point, provided that this is small; but the proportionality does not hold if the latter materially exceeds 1°. In other words, the pressure required to raise the melting-point by 1° does not materially depend on the nature of the dissolved matter, provided that the strength of the solution does not exceed about $2\frac{1}{2}$ per cent. It is a pity that the author does not give the corresponding compensating pressure for 1° in the case of pure benzene. G. H. B.

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499. *Closed Isothermal Reversible Cycle under Gravity.* **A. Ponsot.** (C. R. 120. pp. 993–996, 1895.)—Let A and B be two equal long vertical tubes of unit section communicating at their upper ends, and whose lower ends can be connected by turning on a tap. Let the system be placed in a medium kept at temperature $T < T_0$, the triple point of water. The tap being closed, suppose A to contain water only, B containing water covered with ice forming a kind of piston, and the upper parts of the tubes being filled with vapour of water. Let F and $f$ denote the vapour-pressures of water and ice, $h$ the height of the ice-piston, H the difference of level of the upper surfaces of the ice in B and water in A. Then when the system is in equilibrium,

$F - f =$ (weight of column H of vapour);
$f +$ (weight of column $h$ of ice) $=$ (pressure at which ice melts at temperature T).

The equilibrium will be unaffected by opening the tap, for if a quantity $de$ of water were to flow (say) from B to A, an equal quantity $de$ would evaporate in A and condense in B, and the increased pressure would melt an equal quantity of ice at the bottom of B. Hence, if the pressures at the lower end of the tubes were not in hydrostatic equilibrium, the water could be made to go through a closed cycle, at the same time performing mechanical work in its passage from B to A, and this is contrary to the Second Law. The cycle must therefore be reversible, and if $u$, $u'$ denote the specific volumes of the ice and water, we have

$$h/u = (h - H)/u' + F - f,$$

whence, since $P = h/u + f$, the author finds, on neglecting small quantities in F and $f$, that

$$P(u - u') = RT \log_e (F/f).$$ G. H. B.

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## LIGHT.

500. *Fluted Spectra.* **A. Schuster.** (C. R. 120. pp. 987–989, 1895.)—H. Poincaré (C. R. p. 758) having upheld the view that interference experiments with long difference of path indicated a certain regularity in the vibrations of white light, the author draws attention to the contrary conclusions drawn by Gouy and Lord Rayleigh, and fully discussed by himself in a paper published in the 'Philosophical Magazine' (June, 1894). Poincaré's argument is faulty, in so far as it assumes that the bright and dark bands observed in the spectrum when two interfering vibrations of white light fall on the slit of the spectroscope satisfy a mathematical expression which gives zero intensity for the minimum of light. The ratio of the minimum to the maximum of light is, on the contrary, finite, and equal to $n/(2R-n)$, where $n$ measures the retardation measured in wave-lengths, and R the resolving power of the instrument used. This expression, which is independent of any hypothesis as to so-called regularity of vibration, holds as long as $R>n$; while if $R<n$ the calculation shows that there are no longer any maxima or minima in the spectrum, but that the distribution of intensity is everywhere equal to the sum of the intensities of the separate vibrations. The author points out that the experiments of Foucault and Fizeau are quite in accord with the results of theory. AUTHOR.

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501. *Errors in Photometric Standards.* **E. Liebenthal.** (Zschr. Instrumk. 15. pp. 157–171, 1895.)—The intensity of the light emitted from various photometric standard lamps is influenced by atmospheric conditions. In order to determine the relative merits of the Hefner and pentane lamps, on behalf of the Reichsanstalt, the author examines the variations produced in the two lamps when the pressure and constitution of the air supplied to them is altered. The photometric measurements were made in terms of the light from glow-lamps of constant candle-power. If $y$ is the luminosity of the trial lamp, the corrections for humidity, $CO_2$, O, N, etc., take the form $y=a-bx$, where $a$ and $b$ are constants to be derived from experiments. Considering first of all the Hefner lamp, this formula, as regards the effect of humidity, is

$$y=1{\cdot}049-0{\cdot}0055\,x,$$

where $x$ represents the number of litres of water-vapour present in a cubic metre of the surrounding air. By applying this correction, the illuminating-power of the Hefner lamp is given to within 1 per cent., and probably within 0·4 per cent. of accuracy; at least as regards humidity. A series of experiments extending over a year shows that the illuminating-power of the Hefner lamp during the winter months, in Germany, is 3·5 per cent. greater than in the summer months. Denoting by unity the lamp-power during March, April, May, October, and November, the results

from June to September are about 2 per cent. too small; and those corresponding to December, January, and February are about as much too great. The humidity was measured by an Assmann's psychrometer; but it is pointed out that a properly calibrated hair-hygrometer is capable of giving satisfactory values of $x$. Lamp-power is affected by changes of barometric pressure; if $\Delta y$ is the variation of light-intensity corresponding to $b$ millimetres of mercury, for the Hefner lamp we have

$$\Delta y = 0{\cdot}00011(b-760);$$

which means that a fluctuation of 40 millimetres in atmospheric pressure brightens or dims the flame by 0·4 per cent. Further tests relate to the corrections for the presence of $CO_2$ in the air supplied to the burners. In the case of the Hefner lamp, if $x'$ is the number of litres of $CO_2$ per cb.m. of dry air,

$$y = 1{\cdot}012 - 0{\cdot}0072\,x',$$

that is to say, that one litre, more or less, of $CO_2$ per cb.m. of the air produces a lamp-power change of about 0·7 per cent. The height of the Hefner flame was read off at a cathetometer; and to avoid errors of foreshortening, due to want of perpendicularity of the flame, two images were simultaneously observed, the first being formed by the direct light from the lamp to the cathetometer; and the second was a side view of the same object reflected along the cathetometer axis by a system of mirrors. The next part of the paper refers to the pentane lamp, and to comparisons between this and the Hefner. It is pointed out that a considerable time is required for the pentane lamp to settle down to working conditions; the metal tubes which supply the hot air do not at once assume a constant temperature, so that the flame, after kindling, slowly lengthens, and generally exceeds the proper limit of height, the wick having to be readjusted. The corrections to be applied on account of atmospheric changes are, according to the author, all greater in the case of the pentane lamp than the corresponding values for the Hefner. Using the same symbols as above, the correction for humidity with a pentane lamp is:

$$y = 1{\cdot}232 - 0{\cdot}0068\,x.$$

If the water-vapour increases by an amount equal to 1 litre per cb.m., the lamp-power varies 0·6 per cent. The barometric correction is

$$\Delta y = 0{\cdot}00049(b-760):$$

thus, a change of atmospheric pressure represented by 40 millimetres of mercury produces a 2 per cent. variation in the light of the pentane standard. From a comparison of these results, the Reichsanstalt has decided to give the preference to the Hefner lamp.

R. A.

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502. *Stereoscopic Projections and Lanterns.* **Moëssard.** (C. R. 120. pp. 1108–1110, 1895.)—The stereoscopic lantern combines a right-eye and a left-eye image with the help of two equal crown-

glass prisms adjustably mounted under an angle of 10°. Three dispositions can be adopted. In the ordinary stereoscope, (I.) the edges of the two prisms face one another, right corresponds to right; (II.) if the images are crossed, the edges remain vertical and parallel, but the prisms are turned the opposite way; (III.) the images and prisms with their edges horizontal are placed above one another. The opaque screen in front of the prisms has two semicircular apertures. In case I. the curved sides are outside the vertical diameters, the distance between which is the same as that between the eyes; in case II. inside; in case III. the diameters lie in the same horizontal, and the semicircles would make up a disc. Each prism produces a displacement $\alpha$, for case II. of $2\alpha$. If K is the distance between two homologous points of the two images, D the distance between the eye and the screen, $\frac{K}{D}$ will be the angle which separates the two images for the naked eye. To produce coincidence, there must be $2\alpha = \frac{K}{D}$. Moreover, to render the illusion perfect for the audience of a hall, for which K and D change, $2\alpha$ must be varied. The holders of the prisms are for this purpose connected with two toothed sectors, which impart to the prisms equal, but opposite rotations, so that the virtual images describe equal circles in opposite directions. If the holders are turned through $\omega$, the apparent relative displacement of the two images will be greater than $2\alpha \cos \omega$, and the condition for accommodation,

$$2\alpha \cos \omega = \frac{K'}{D'}$$

will remain possible, as long as $\frac{K'}{D'} < \frac{K}{D}$. When looking through the instrument, two useful images will be seen in the centre, two others on the wings; the holders are turned until the former coincide. The ordinary juxtaposition I. of the stereoscope is not advisable, as the images are difficult to separate and apt to produce bands in the plane of the eye. The crossed arrangement II. is preferable; the superposition III. is particularly suitable for panoramic views of great width. H. B.

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503. *Spectroscopical Observations of Saturn's Rings.* **H. Deslandres.** (C. R. 120. pp. 1155–1158, 1895.)—Same method applied as in the observations on Jupiter (C. R. Feb. 1895). Results anticipated in the main by Mr. Keeler (Astrophysical Journal, May 1895), who has used the same method; but the author also determined the relative velocities in the ring. The exterior ring has a less velocity than the inner, by 4·7 kilometres. The limb, at distance 1 from the centre, has a velocity of 9·38 km. (calculated 10·3); the inner ring, at distance 1·5, velocity 20·10 (calc. 21·0); the outer ring, at distance 2·2, velocity 15·40 (calc. 17·14). The result does not, however, strictly prove the meteoric nature of the ring; for it does not prove tangential division, and

it proves normal division only up to a certain point, so long as the great divisions of Cassini do not appear upon the photographic plate. A larger instrument is required, giving a larger image. The Döppler principle operates twice—once in respect of the sun, and once in respect of the earth; so that the observed displacements correspond to twice the actual velocities. Diffusion of light at the surface of the planet does not appear to have any effect on the wave-length. A. D.

504. *A Mirror Cœlostat.* **G. Lippmann.** (C. R. 120. pp. 1015-1019, 1895.)—In a siderostat, the image of one star appears stationary, whilst the other stars seem to turn about it. A cœlostat would show the whole firmament stationary. A mirror is mounted on strong supports, its axis parallel to the polar axis, and turned uniformly once in 48 sidereal hours in the direction of the movement of the stars. If EO is the direct ray, and E′O the production of the reflected ray, and the plane of the mirror parallel to the polar axis, then OP, also parallel to this line, will make equal angles EOP and E′OP. EOP is the polar distance of E, and independent of the hour; the plane EOP turns about OP with a velocity $\alpha$ (diurnal movement), the plane of the mirror revolves with velocity $\beta$; E′OP, therefore, has a velocity $(2\beta-\alpha)$ about OP; but $\beta=\frac{1}{2}\alpha$ (one revolution in 48 hrs.): hence $2\beta-\alpha=0$, that is to say, the plane E′OP remains stationary, and E′O, the ray observed, likewise. The paper proves that under certain circumstances a siderostat becomes a cœlostat, and that there is only one type of cœlostat possible. It may serve as equatorial. Several constructive points are discussed, but it is not said that construction of such an instrument is intended. H. B.

505. *Dissociation and Optical Rotation.* **E. Rimbach.** (Zschr. phys. Chem. 16. pp. 671–676, 1895.)—An attempt to establish experimentally a relation between grades of dissociation as deduced from (1) electrical, and (2) optical measurements. The former are made in the usual way by measuring the resistances of solutions of a salt by Kohlrausch's method. By extrapolation a value of the molecular conductivity $\mu_\infty$ for infinite dilution is deduced. For any other degree of dilution the (percentage) electrolytic dissociation is given by $100 \,.\, \mu/\mu_\infty$. In dealing with the measurements of optical rotation, it is similarly assumed that the value of the specific rotation for infinite dilution $\alpha_\infty$ can be found by extrapolation from values at known dilutions. A more doubtful extrapolation is employed to deduce the supposed specific rotation $\alpha_0$ for infinite concentration (*i. e.*, for the salt in the amorphous solid state) from observations on fairly concentrated solutions. For any other solution of the same salt producing a rotation $\alpha$, the degree of dissociation as deduced from the optical measurements is given by $100(\alpha-\alpha_0)/(\alpha_\infty-\alpha_0)$. The salt chosen for experiment is rubidium tartrate, which is very soluble in water (100 parts of water dissolve about 200 parts of the salt). The numerical

results obtained by the two methods do not agree well, those given by the optical method being always considerably higher than those given by the electrical method. If the two sets of dissociation-values are represented graphically as functions of the percentage composition of the solution, we get two curves which are approximately parallel. Thus, for solutions of moderate concentration, the relative change in the rotation is proportional to the change in the electrolytic dissociation. D. E. J.

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506. *Infra-Red Dichroism.* **E. Merritt.** (Phys. Rev. 2. pp. 424–441, 1895.)—This paper deals with the absorption by tourmaline, quartz, and Iceland spar in the infra-red as dependent upon the direction of the plane of polarisation. A selective absorption of the ordinary or extraordinary ray may occur even when no dichroism is observable in the visible spectrum. It may be expected that all doubly-refracting crystals are dichroic, in this sense, for some part of the total spectrum. Uniaxial crystals only were used in the present experiments, and the absorption on transmission parallel and perpendicular to the optic axis was measured by means of a Rubens spectro-bolometer. The results are summarised as follows:—Observations extending to $\lambda=4{\cdot}5\,\mu$ in the case of quartz and tourmaline, and to $\lambda=5{\cdot}5\,\mu$ in the case of Iceland spar, show that the absorption of polarised rays is greatly influenced by the direction of the plane of polarisation in all three crystals. The transmission-curves for the ordinary and extraordinary rays appear, in fact, to be entirely independent of one another. The difference between the two curves is most marked in the case of Iceland spar and tourmaline. Absorption-bands are found in the infra-red transmission-spectra as follows:—

*Iceland spar*: Ordinary ray, $2{\cdot}44\,\mu$ and $2{\cdot}74\,\mu$. These bands are quite sharp. Broad bands are shown at $3{\cdot}3\,\mu$, $4{\cdot}0\,\mu$, and $4{\cdot}6\,\mu$. Extraordinary ray, $3{\cdot}28\,\mu$, $3{\cdot}75\,\mu$, and $4{\cdot}66\,\mu$.

*Quartz*: Ordinary ray, $2{\cdot}90\,\mu$. Extraordinary ray, $3{\cdot}00\,\mu$. Neither band is very sharp.

*Tourmaline*: Ordinary ray, $2{\cdot}82\,\mu$. In the case of tourmaline the transmission-curves intersect at $2{\cdot}30\,\mu$ and $3{\cdot}48\,\mu$, so that between these two points the dichroism is reversed.

Determinations of percentage transmission in the infra-red are shown to be liable to error on account of a slight scattering of the rays on passing through the prism. An impure spectrum results from the presence of these diffuse rays, which may lead to serious error at points beyond $2{\cdot}5\,\mu$. Such errors may be avoided without great loss of sensitiveness by using two spectrometers, one of which throws its spectrum upon the slit of the other. E. E. F.

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507. *Cobalt and Chromium Absorption-Bands.* **A. Étard.** (C. R. 120. pp. 1057–1060, 1895.)—The absorption-spectra of solutions of salts of chromium and cobalt were studied in order to test whether the atom or the molecule was the factor determining

the position of the absorption-bands. Chromium chloride gives a broad band extending from D to E. The sulphate, the nitrate, and the alum show a characteristic band at $\lambda = 678$–$670$. The addition of a nitrite turns them reddish-violet, and an arseniate turns them green. But the characteristic band is only slightly displaced towards the red, into the position $\lambda = 687$–$680$, where it is also found in the case of concentrated chromic acid. On the other hand, anhydrous chromyl chloride, the chromate and bichromate of potassium, and the roseochromic sulphate give no distinct band with the same thickness, whereas the blue oxalate of chromium and potassium shows a very striking spectroscopic appearance in the form of an intensely black band of $\lambda = 700$–$693$ in a rectangle of red light extending from 736 to 680. It is evident, therefore, that it is not the atom which determines the absorption-spectrum, but the molecule of combination—violet, green, or yellow. In the case of cobalt the author studied the change from pale red to blue undergone by a solution of cobalt chloride with the addition of a little hydrochloric acid on raising the temperature. Between 66° and 67° C., and in the interval of one degree, the band extending from 655 to 649 suddenly appears, and another between 687 and 680 appears at 80° or 85°. The spectrum is the same as in the red state, with the addition of the two bands. The author concludes that the salts of chromium and the red salts of cobalt possess, in the same way as the rare earths and the salts of uranium, well-defined spectrum bands; and that the spectra of these metals are nevertheless spectra of molecules, like those furnished by organic substances such as chlorophyll. The hypothesis according to which a different element should correspond to each band in the spectrum of a rare earth is not necessarily true, as shown by the behaviour of cobalt. The bands may be considerably displaced or may disappear for the same element, according to the nature of the molecules in solution or the compound observed. E. E. F.

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508. *Photometry of Mean Spherical Power.* **A. Blondel.** (L'Ind. El. No. 81, pp. 185–186, May 10, 1895.)—The author describes several methods of measuring the total flux of light, or the mean spherical candle-power of a source of light. In one method he proposes to collect the whole light by means of hemispherical and parabolic or elliptical mirrors, and to throw it on a diffusing-screen beyond which the photometer is placed. To avoid the difficulties of this method, especially the variation of reflecting power at different angles, the author encloses the light in an opaque globe from which portions between two meridians have been removed. If the source of light emits its rays symmetrically about a vertical axis, the edges of the openings in the globe are arranged in vertical planes. The light escaping through the openings falls on a zonal reflector of polished metal or silvered glass, or on a conical reflector of depolished enamelled iron, and thence to the photometer. The apparatus is called a "lumen-mètre." A. P. T.

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509. *White Light.* **A. Garbasso.** (N. Cim. 4. 1. pp. 305–307, 1895.)—The author endeavours to show that white light has its origin in simple, strongly damped sinusoidal vibrations, and points out that from this hypothesis follows a distribution of energy in the spectrum which is perfectly similar to that obtained when studying solar radiation by the bolometer. On working out the influence of damping by the analogy of electric oscillations, the author finds that the fundamental vibration of the solar spectrum would have the wave-length $0{\cdot}659\,\mu$, whereas its maximum of intensity would correspond to a wave-length of $0{\cdot}60\,\mu$. On reducing the scale of the curve obtained to that of Langley's solar-radiation curve, it is found that the two curves coincide almost perfectly in the ultra-violet and the visible spectrum, whilst the new curve is slightly higher than Langley's in the infra-red. The bright line $H\alpha$ of the hydrogen spectrum corresponds to waves of length $0{\cdot}656\,\mu$, and hydrogen placed under conditions such that its vibrations have the logarithmic decrement 2 will emit light whose spectrum will be similar to that of the sun. E. E. F.

# HEAT.

510. *Proof of the Boltzmann-Maxwell Law.* **M. Planck.** (Wied. Ann. 55. pp. 220–222, 1895.) *The Maxwell Law.* **L. Boltzmann.** (Ibid. pp. 223–224.)—It had been shown by Boltzmann in a previous note that the proof of the law of distribution of velocity given in Planck's edition of Kirchhoff's 'Lectures on the Theory of Heat' (p. 142) was wanting in rigour. Planck, after pointing out that the proof in question was taken from Kirchhoff's lectures themselves, and was not due to him, endeavours to meet Boltzmann's objections by deducing the law of distribution from the principle of reversibility. He concludes that "the Maxwell Law is the only law of partition of velocity which accords with the principle of mechanics that the dynamical conditions of equilibrium of a system of particles is unaffected by suddenly reversing the velocity of every particle."

In his reply, Boltzmann refers to the recent discussion in 'Nature,' pointing out that the principle of reversibility could not be applied (as Planck had done) to *prove* the law of distribution of velocity without making further assumptions, since this same principle has also been employed as a test of the assumptions involved in the proof of the Minimum Theorem. If, however, we assume the exclusion of "ordered motions" among the molecules of a gas, so that collisions take place *at random*, according to the ordinary laws of probability, and if the distribution be permanent or stationary, it must remain stationary when every velocity is reversed, so that the probabilities of "direct" and "reverse" collisions must be equal, and the Boltzmann-Maxwell Law at once follows. G. H. B.

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511. *Some Relations between Temperature, Pressure, and Latent Heat of Vaporisation.* **C. E. Linebarger.** (Am. J. Sci. 49. pp. 380–396, 1895.)—In addition to the well-known formula of thermodynamics

$$\frac{l}{\theta}=(v-v')\frac{dp}{d\theta},$$

or with the present author's notation

$$\frac{dp}{dT}=A\frac{\rho}{v-v'},$$

the latent heat of vaporisation $l$ or $\rho$ is known, as the result of experiment, to satisfy certain other empirical relationships, and the present paper is taken up with a consideration of the evidence, both theoretical and experimental, in favour of these. The author considers especially the law known as "Trouton's Law," which, however, had been enunciated by Pictet in 1876, eight years before Trouton's paper, according to which $\mu\rho/T$ is constant at constant pressure ($\mu$ being the molecular mass and T the temperature of the boiling-point). Unfortunately, the mathematical formulæ are so

full of misprints and errors that the theoretical part of the paper is worthless; this applies particularly to the account of Le Chatelier's work, in which (according to the present writer) $\rho$ is first assumed to be independent of T and is hence proved to be proportional to T! The author gives tables of the values of $\mu\rho/T$ for different substances at atmospheric pressure as determined by different experimenters, and says :—" Taking all the reliable determinations into consideration we find that the average value of the 'constant' is for about seventy liquids equal to 20·70, the greatest value being 22·04 for bromine (Andrews). For the elements and inorganic compounds the 'constant' is equal to 20·41, with 22·04 and 19·66 as extreme values; for the hydrocarbons 20·19, 20·63 and 19·58 being the extreme values; for the halogen compounds 20·63, with extreme values equal to 21·16 and 19·59; for the esters 20·87, the extremes being 21·43 and 20·36." It is suggested that the deviation from constancy in the value of $\mu\rho/T$ is due to the molecules of the liquid being in a state of "association." "If any liquid whose latent heat of volatilisation be known gives a value of the 'constant' close to 20·7, it is pretty certain that it is 'normal.' If it gives a less value, it is associated in the liquid as well as in the gaseous state; if it gives a greater value, it must be associated in the liquid state alone. The greater the variation from the normal value of the 'constant,' the greater the amount of the association."

The dependency of the latent heat on the pressure is next considered, but as Le Chatelier's doubtful formula is assumed, the author's generalisations must be regarded with suspicion. G. H. B.

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512. *Experimental Determination of the Density of Saturated Vapours.* **G. Bauer.** (Wied. Ann. 55. pp. 184–212, 1895.)—The method adopted was that of the "aerostatic" balance. A thin copper balloon of about 1 litre was hung by a thin iron wire to one arm of a balance; round it was placed a large double-jacketed vessel, through which vapour from a boiler could be passed. The alteration of weight showed the density of the vapour. In the experiments the balloon was first heated above the boiling-point of the liquid, so that no vapour should be condensed on it. Its volume was determined by loading it with shot and weighing in water. The suspending wire passed through a hole 2 mm. wide in the lid of the steam vessel; and to avoid condensation of the vapour on the wire above the hole, a small gas-flame was made to play on it in the case of water-vapour; that also served to keep the vapour from being condensed on the lid, and so possibly dropping on to the balloon: when chloroform and other vapours were used, a gentle stream of air was sucked in through the hole in the lid, and away by a side tube, down which the vapour was carried off. There was an arrangement to allow the vapour to be let in either at the top or bottom of the balloon; no difference of weight was observed in the two cases. Experiments were made at three different stations, where the pressure averaged 72, 62, and 52 cm,

respectively. The author's results are in fair agreement with Zenner's formula for water-vapour (which was derived from the latent heat and vapour-pressure), viz.,

$$\text{density} = 1{\cdot}1929\, p^{0{\cdot}9303} \text{ gms. per cubic metre,}$$

where $p$ is the pressure in mm. of mercury; but he finds they are better represented by the linear formula

$$\text{density} = {\cdot}8101\, p.$$

Also measurements of sulphur dioxide, carbon tetrachloride, chloroform, ether, and alcohol agreed with Zenner's theoretically determined values. R. A. L.

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513. *Dilatation of Aqueous Solutions.* **C. Forch.** (Wied. Ann. 55. pp. 100–120, 1895.)—A painstaking work on the dilatation, between 0° and 40°, of solutions of $HNO_3$, $LiNO_3$, $NH_4NO_3$, $NaNO_3$, $KNO_3$, $H_2SO_4$, $Li_2SO_4$, $KHSO_4$, $K_2SO_4$, $ZnSO_4$, $CuSO_4$, $H_3PO_4$, $KH_2PO_4$, $K_2HPO_4$, $K_3PO_4$, KBr, KI, NaOH, KOH, and $ZnCl_2$. The concentrations were generally so chosen that the solutions contained approximately $\frac{1}{2}$, 1, and 2 gramme-equivalents in the litre. The measurements were made by means of a dilatometer; the author calculates that under the most unfavourable circumstances the errors in the values of the coefficients of expansion could not exceed two per cent. Of interest is a magnetic stirrer placed under the liquid and worked by magnetic influences from without. Complete and copious tables are given. A. Gs.

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514. *Measurement of High Temperatures with the Thermo-Element and the Melting-Point of some Inorganic Salts.* **J. McCrae.** (Wied. Ann. 55. pp. 95–99, 1895.)—By means of a thermo-element made of platinum and rhodium-platinum the author determines the temperature of an alcohol flame, the summit of the blue cone of a non-luminous Bunsen flame, the hottest point of a Bunsen flame, the boiling-point of $SnCl_2$, the melting-points of NaI, KI, KBr, NaBr, $CaCl_2$, KCl, NaCl, $SrCl_2$, $Na_2CO_3$, $Na_2SO_4$, $K_2CO_3$, $BaCl_2$, and $K_2SO_4$; by means of an element of platinum and iridium-platinum the melting-points of NaI, KI, KBr, $Na_2CO_3$, $K_2CO_3$, $BaCl_2$, and $K_2SO_4$. He compares his results with those of Victor Meyer, Riddle and Lamb, Le Chatelier, and Heycock. A. Gs.

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515. *Thermometer with a Constant Freezing-Point.* **L. Marchis.** (J. Phys. 4. pp. 217–219, 1895.)—The thermometer consists of a platinum bulb of the ordinary form welded to a thick glass stem. The bulb is protected by a double shield of glass and platinum gauze. The liquid used is mercury. Difficulty is experienced in filling the thermometer, since mercury raised to the temperature of its boiling-point attacks platinum. On this account air-bu bles have to be got rid of mechanically. The zero of this therm meter is remarkably constant. W. G. R.

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516. *Specific Heat of Mercury between* 0° *and* 30°. **A. Bartoli** and **E. Stracciati.** (N. Cim. 4. 1. pp. 291–299, 1895.)—The results of previous experimenters are very divergent, some indicating that the specific heat of mercury increases with rise in temperature, others that it diminishes. The authors' results confirm those of Naccari and Winkelmann, which indicate that there is a diminution. The caloric capacity of mercury is compared with that of water by the mediation of platinum; a mass of platinum is raised to a temperature of about 100°, dropped into water at various temperatures, and the rise in temperature noted, etc. Similarly with the platinum and mercury. The results obtained are indicated in the following table, where T represents the temperature and $C_T$ the specific heat at temperature T, *the specific heat of water at* 15° C. *being taken as unity*:—

| T. | $C_T$. | T. | $C_T$. |
|---|---|---|---|
| 0° | 0·033583 | 16° | 0·033527 |
| 1 | 583 | 17 | 520 |
| 2 | 583 | 18 | 512 |
| 3 | 581 | 19 | 497 |
| 4 | 580 | 20 | 0·033493 |
| 5 | 0·033579 | 21 | 483 |
| 6 | 578 | 22 | 472 |
| 7 | 576 | 23 | 462 |
| 8 | 573 | 24 | 447 |
| 9 | 569 | 25 | 0·033432 |
| 10 | 0·033563 | 26 | 417 |
| 11 | 557 | 27 | 402 |
| 12 | 551 | 28 | 388 |
| 13 | 546 | 29 | 367 |
| 14 | 540 | 30 | 0·033348 |
| 15 | 0·033533 | 31 | 331 |

The results can be expressed by the two empirical formulæ, (A) and (B), given below:—

$$(A) \quad C_T = 0{\cdot}033583 - 0{\cdot}000000333\,T - 0{\cdot}000000125\,T^2 - 0{\cdot}000000004165\,T^3;$$

$$(B) \quad C_T = 0{\cdot}033583 + 0{\cdot}00000117\,T - 0{\cdot}0000003\,T^2.$$

Finally there is given a table which compares the authors' determinations with those of Naccari and Winkelmann. A. Gs.

# ELECTRICITY.

517. *Quadrant Electrometer as a Differential Instrument.* **H. Eisler.** (El. Z. Ht. 17. 1895.)—A valuable paper, very similar to an article by Arnò, with practically the same title, of which an abstract was given in No. 452 of last month's Journal. The author points out the resemblance between his own ideas and Arnò's, and explains that his own paper was sent to the El. Z. some weeks before he obtained an account of Arnò's work. A. Gs.

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518. *New Method of Measuring Specific Inductive Capacity.* **A. Nodon.** (L'El. 9. pp. 305–306, 1895.)—Two fixed spheres of small capacity, and at some distance apart in air, are subjected respectively to a high potential and a rapidly alternating potential; a third sphere, also of small capacity, is placed between them; one terminal of a telephone is connected to this sphere, and the other terminal is earthed. For some position of the third sphere there will be silence in the telephone: on placing a slab of paraffin between one of the fixed spheres and the moveable one, a new position of silence is obtained. By replacing the paraffin by a similar slab of another dielectric, the moveable sphere will have another position for silence in the telephone. If $I_1$ and $I_2$ are the specific inductive capacities of two dielectrics, and $d_1$ $d_2$ the corresponding distances of the respective positions of silence from the position of silence in air, then

$$I_1 : I_2 = d_1 : d_2,$$

and the specific inductive capacities of the dielectrics can be compared. Paraffin is taken as the standard dielectric. The method is applicable to liquid and gaseous dielectrics as well as to solids, and is susceptible of considerable accuracy. W. G. R.

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519. *Electric Discharge in Argon and Fluorine.* **H. Moissan.** (C. R. 120. pp. 966–968, 1895.)—When sparks are passed through a mixture of argon and fluorine, there is no apparent combination; but the volume of the mixture before and after sparking was not determined. S. R.

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520. *Atmospheric Electricity on the Eiffel Tower.* **Chauveau.** (Bull. Soc. Fr. Phys. No. 62. pp. 3–4, 1895.)—The author has improved Mascart's registering instruments. He has regulated the flow from the water-cistern, charges the sectors of the electrometer with a Damien cell (sulphate of magnesia), and avoids the zero displacement—the great drawback of Mascart's electrometer—by floating a little cup filled with glycerine and weighted with mercury in the vessel containing the sulphuric acid; the arbor of the needle dips into the glycerine, which has a platinum connection

with the acid. Instead of Mascart's somewhat cumbrous registering clockwork, he employs a cylinder made by Richard upon which photographic paper is rolled, the sensitised surface being inside. The cistern is fixed to one of the four arches which bear the great lamp of the third platform; the nozzle projects about 1·50 m. Although the jet is thus produced within a surface connecting the lateral and the central lightning-conductors, the electrical phenomena are 25 times as strong on the ground, and the pressure frequently exceeds 8000 volts. The insulation causes no trouble in the dustless air on the top of the Tower. But the determination of the potential-differences is doubtful. Hopkinson first pointed out that the indications of quadrant electrometers appear to have a limiting value near 3500 volts. According to the author, no theoretical explication of this peculiarity has as yet been given: he ascribes it simply to an electric couple and a want of symmetry in the combination of the needle and the sectors. By interposing small well-insulated condensers, he believes to have overcome the difficulty of having to deal with inconveniently high electromotive forces. H. B.

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521. *Electricity Meter.* **Harries** and **Riemann.** (El. Rund. 12. pp. 145–146, 1895.)—A cylinder, rotating about a vertical axis, is provided with projecting ribs, or pinions; these are of different lengths, so that the cylinder is equivalent to a series of toothed wheels having a diminishing number of teeth counting from the base upwards. A star-wheel carried on a vertical rod comes into gear with the pinions; the amount of rotation of the star-wheel thus depends upon its height above the base of the cylinder, and this distance is regulated by the action of a solenoid. The revolutions of the star-wheel are recorded by the usual mechanism of toothed wheels and dial indicators. R. A.

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522. *Extremely Sensitive Galvanometer.* **P. Wiess.** (J. Phys. 4. pp. 212–216, 1895.)—The sensitiveness of the suspended magnetic system is proportional to M/I, where M is the magnetic moment and I the moment of inertia of the suspended system about a vertical axis. To increase this ratio as far as possible, the magnets are formed of two long thin needles arranged vertically with dissimilar poles adjacent so as to be astatic. They are fixed to the edges of a narrow strip of mica, to the bottom of which the mirror is attached. The four galvanometer bobbins are arranged with their axes passing through either the top or bottom ends of the magnets. In one instrument the steel needles are 0·2 mm. in diameter and 18 mm. long, and are placed 1·2 mm. from each other. The mirror measures 7 mm. by 2 mm., and weighs 8 mg. Only one size of wire is used on the coils. Such an instrument is said to be as sensitive as one of a much more elaborate construction made for the Washington Observatory, and described by Wadsworth in the Phil. Mag. vol. 38. p. 553, 1894. W. E. S,

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523. *Duration of Electrical Shadows caused by Solid and Fluid Insulators.* **G. Quincke.** (Berl. Ber. 28. pp. 525–531, 1895.)—The shadows referred to are those produced when a body is placed between two electrodes across which a glow-discharge is passing. One of Quincke's electrodes consists of a conical point, the other of a brass plate 20 cm. diameter covered with a layer of pure white silk, their distance apart being 8 cm. To measure the duration of a shadow, the body is placed between the electrodes and left in the same position until the shadow has disappeared; it is then revolved about a vertical axis through an angle of 180°, when the shadow at first reappears, but ultimately disappears; the duration of the shadow is defined as the interval between its reappearance and disappearance. Five tables are given, which include observations on calc-spar, quartz, arragonite, sulphur, various sorts of glass, turpentine (the liquids were contained in flint-glass tubes), petroleum, ether, carbon bisulphide, benzol, water, alcohol, chloroform, mercury, etc. Comprehensive generalisations are not made, but interesting observations, of a more or less isolated character, are given. With paraffin oil the shadows never completely vanish. The duration of the shadow caused by mercury, *under the given conditions*, is remarkably long. The long duration of electrical shadows with insulating fluids corresponds to the slow production of electrical double refraction, often previously observed by the author. The increase of duration on heating glass is perhaps connected with the increase of electrical conductivity. A mixture of turpentine and carbon bisulphide produces a shadow of much longer duration than either separately: probably owing to the time taken by the electric forces to separate, to some extent, the two liquids. Calc-spar, quartz, arragonite, and sulphur behave differently according as their optical axes are parallel or at right angles to the lines of force. A. Gs.

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524. *Microscopy of Spark-Gaps.* **G. T. Hanchett.** (El. World, 25. pp. 607–608, 1895.)—When two strips of tin-foil are laid upon a glass plate, and small discharges are passed between them, they lose very little in weight; but an impalpable dust is deposited, which is the more profuse at the positive terminal. The author examines the spark-gap microscopically, and observes two principal lines of discharge, one of which is blue and the other red. These lines have, respectively, opposite characteristics. If the discharge is so weak as scarcely to be able to jump the gap, the red line is not visible; with increasing potential, the red appears twisted among the blue lines; by still further increasing the potential it broadens, and at the same time shortens, so that it does not extend now to the negative terminal; finally, it takes the form of a red mist about the blue line. The blue portion of the discharge is assumed to carry most electricity; it is the first to appear when the gap is shortened to sparking distance; it spreads itself over the negative terminal. When the spark is covered by a thin glass plate

the blue almost disappears, but the red becomes more brilliant, more sharply defined, and less like a mist or glow. The first result of introducing additional capacity into the circuit is to strengthen the blue line; but the blue grows whiter as the capacity is further increased. A continued addition of capacity causes the haze to turn from red to yellow, and this yellow mist completely envelops both ends and sides of the discharge. R. A.

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525. *Specific Inductive Capacities.* **D. Mazzotto.** (N. Cim. 4. 1. pp. 308–310, 1895.)—The specific inductive capacities of petroleum, sulphur, paraffin, olive-oil, and plate-glass were determined by a method similar to that employed by Lecher. Placing the condenser at the terminals of the secondary wires of Lecher's apparatus, the position of the node of the fundamental vibration was found along these wires; or the nodal lines of the system were found by the method previously described. To find the capacity of the terminal condenser, two methods were employed. Either it was calculated from Cohn and Heerwagen's formula, or the capacity of the air-condenser was found which, when substituted for the condenser containing the substance experimented upon, did not change the duration of the vibration. The mean values of the specific inductive capacities found are:—petroleum 2·11, sulphur 2·68, paraffin 1·68, olive-oil 2·87, plate-glass 3·76.

E. E. F.

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526. *Sparking Distance.* **G. Jaumann.** (Wien. Ber. 104. Part II *a*. pp. 7–36, 1895.)—In consequence of the oscillatory nature of electric discharge, and the resulting fluctuations of the surrounding field, there are certain conditions of instability at a spark-gap: the spark-length is not a simple linear function of the potential between the terminals, but it is determined by the material of the electrodes, the nature of the dielectric, and the time-rate of change of the potential. If the conditions of discharge are such that the inevitable oscillations are very great or very rapid, there will be little variation in the potential corresponding to a given gap. This is realised approximately when an "influence" machine is used as the generator. With weaker discharges Jaumann finds that the measured potential may be as much as four times the value calculated from the length of gap. His apparatus consists of two circular plates, 18 cm. radius, forming a condensing system; these are connected, respectively, to two small spheres of finely polished platinum. Having charged the condenser to a given potential, the spark-gap is diminished until the discharge occurs. It is noticed that this gap is remarkably short for the potential between the spheres. When, however, the spark-gap is kept constant, and the plates are separated until the potential rises to the sparking limit, the corresponding sparking distance is much longer than the calculated value. The potential is measured by an indicator, which has been calibrated by an absolute electrometer.

The effect produced upon the spark-transmitting properties of a gap by the mere passing of the sparks, which is usually ascribed to the corrosion of the electrodes or the heating of the dielectric of the spark-field, is also examined. It appears that in some cases the spark-transmission may be impaired, and in other conditions it may be improved, by the passage of sparks; small sparks are more susceptible to changes of the first kind than are larger ones. The passing of weak sparks may sometimes improve the transmitting properties of a gap; and an impaired spark-gap occasionally improves after a prolonged series of discharges. That these effects are not due to the presence of dust, was proved by directing a stream of dust-charged dry air between the terminals. This unstable condition of the spark-gap is associated with the phenomenon of the discharge of an electrified body by the action of light. If care is taken to avoid rapid fluctuations of potential, it is generally possible to attain a very high potential before a spark jumps the gap; there is, in fact, a retardation akin to the lag in the boiling and solidification of certain liquids: when the conditions of instantaneous discharge are not quite fulfilled, the discharge may occur after an interval of a few seconds, or even minutes; during this critical period of delay there is no loss of charge, but there may be fluctuations of potential, the preliminaries of the ultimate disruption. R. A.

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527. *Capacity of Electrolytic Condensers.* **S. Sheldon, H. W. Leitch,** and **A. N. Shaw.** (Phys. Rev. 2. pp. 401–411, 1895.)—The authors determine experimentally the capacities of condensers having respectively platinum and mercury electrodes in sulphuric acid. Difficulty is met with owing to the polarisation of the cells; even after short-circuiting the cells for a considerable time, there is marked polarisation. To avoid delay in bringing the electrodes into a neutral condition, they employ a "method of reversals" similar to that used in testing iron. They impress, for a certain time, a difference of potential upon a circuit containing the electrolytic cell in series with a non-inductive resistance equal to the resistance of the ballistic galvanometer used. The polarity is reversed by means of a commutator, and the galvanometer is substituted for the resistance. The throw of the galvanometer gives twice the quantity of electricity necessary to depolarise the cell from a given voltage to zero. The capacity at that voltage is the quotient of quantity by voltage. In studying a Pt—$H_2SO_4$ cell the authors find that the capacity varies from a certain definite value for small E.M.F.'s of polarisation to infinity at 2·62 volts. Taking the formula

$$\frac{1}{C}=\frac{1}{K}\left(\frac{1}{S_1}+\frac{1}{S_2}\right)$$

from which to calculate the capacity, where C=capacity, K=a constant, and $S_1$ and $S_2$ are the respective areas of the electrodes, they determine the values of K at different temperatures for both

Pt—$H_2SO_4$ and Hg—$H_2SO_4$ cells. For the effect of temperature on the capacity of a Pt—$H_2SO_4$ cell, they give the formula

$$C = C_{18}\{1+(t-18)\,0{\cdot}0215\},$$

where $C_{18}$ = capacity at 18° Centigrade. The Hg—$HS_2O_4$ cells have a larger temperature-coefficient. It is proved experimentally that the usual laws hold good when electrolytic condensers are coupled in series or in parallel. Considering the efficiency of a condenser to be the ratio of the energy of discharge to that of charge, it is found that the efficiency of electrolytic condensers is small. The authors conclude that electrolytic condensers are unsuitable for annulling self-induction, and that their high temperature-coefficient and low efficiency preclude their adoption for practical purposes. W. G. R.

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528. *Resonance in Transformer Circuits.* **F. Bedell** and **A. C. Crehore.** (Phys. Rev. 2. pp. 442–454, 1895.)—In a single circuit electrical resonance is obtained when the resultant reactance is zero. If the circuit contains self-induction I and capacity C, the condition for resonance is

$$Ip = \frac{1}{Cp},$$

where $p=2\pi n$, and $n$ is the frequency of the applied E.M.F. Effects corresponding to resonance in a single circuit may be produced by the reactive influence of one circuit upon another, occasioned by the mutual induction of the two circuits. If we consider the primary and secondary coils of a transformer, resonance will occur when the apparent reactance of the primary circuit is zero. Confining the term *Resonance* to a single circuit, the corresponding effect with two circuits is called *Consonance.* The authors determine analytically and graphically the conditions which must be satisfied by the constants of the primary and secondary coils of a transformer in order that consonance may occur. The apparent reactance of the primary circuit

$$= K_1 - \gamma^2 K_2,$$

where $\gamma=\dfrac{Mp}{I_2}$,

$K_1$=primary reactance,
$K_2$=secondary reactance,
M=coefficient of mutual induction of the two circuits,
$I_2$=secondary impedance.

Consonance therefore occurs when

$$K_1 - \gamma^2 K_2 = 0;$$

that is, when

$$\frac{K_1}{K_2} = \gamma^2 = \frac{M^2p^2}{I_2^2} = \frac{M^2p^2}{R_2^2+K_2^2},$$

$R_2$ being the secondary resistance. This is a quadratic equation in $K_2$, the solution of which is

$$K_2 = \frac{M^2 p^2 \pm \sqrt{\{M^2 p^2 - 4K_1^2 R_2^2\}}}{2K_1}.$$

Thus, when the primary reactance is fixed, there are two values of the secondary reactance for which consonance will occur. $K_2$ must be real; that is, for consonance to be possible at all, we must have

$$M^2 p^2 > 4K_1^2 R_2^2.$$

The apparent reactance of the primary coil will also be zero if

$$K_1 = K_2 = 0.$$

The authors call this case "pure consonance," since it occurs when the natural period of each circuit independently is equal to the period of the impressed E.M.F., and the period of the system is the same as that of either circuit. W. G. R.

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529. *Electrical Observations on the Sonnblick.* **J. Elster** and **H. Geitel.** (Wien. Ber. 104. Part II *a*. pp. 37–45, 1895.)—This is published now because the observer, Peter Lechner, has had to take up other work. The report tabulates the potentials from October 1893 to March 1894, and St. Elmo's fires from February 1893 to May 1894. The potentials, formerly determined every hour, are now derived from observations at $7^h$, $2^h$, $9^h$, which gave the same daily means. The paper states the monthly means of the years 1890–93 or 1891–94, volts, number of observations, temperature, vapour-tension, cloudiness. They are all in fair agreement. In January 1894 only 79 volts were observed against 122, 148, 134 of the three preceding years; no such difference was found at Wolfenbüttel: the cause must hence be local. The volts are referred to a unit dependent upon the locality, and are therefore comparable amongst one another, but not to the unit volt/meter. The Sonnblick projects into atmospheric strata which show little of the periodical variations in the lower strata. These variations the authors formerly ascribed to negative electricity. Recent balloon observations seem to prove, however, that up to +3000 m. positive electricity prevails. On the whole they regard, with Kelvin and with Trabert, the earth as a condenser, and account for the decrease in the potential with the rising sun by photo-electrical discharges, without being able to say why the potential varies, for each locality, with the seasons but remains constant on average. The St. Elmo's fire observations, generally taken every ten minutes, were during the summer months often disturbed by visitors. The discharge is negative with dusty snow, positive when flakes fall, and changes when both kinds alternate. Hail is accompanied by very strong positive discharges. The temperature rose only in July and August to +5°·4; during the other months it kept below zero, the minimum tabulated (−23°·6) occurring on March 19, 1893.

H. B.

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530. *Thermoelectric Power and Elongation of Magnetised Iron.* **P. Bachmetjew.** (Wien. Ber. 104. Part II*a*. pp. 71–85, 1895.)—The author has shown that the thermoelectromotive force between magnetised and unmagnetised iron is proportional to the square of the magnetisation (for moderate fields); Nagaoka has shown that the magnetic elongation follows the same rule. These two quantities are therefore proportional to one another. In the present paper the author shows that the thermoelectromotive force in a magnetised wire varies from zero in the middle to a maximum at about 15 per cent. of the length from each end, and falls off again a little towards the end. The elongation, and therefore the internal work done, must vary similarly. This is in agreement with previous experiments of the author, showing that the production of heat in magnetisation is greatest in the middle of the wire. Experimental details are given, but present no special novelty. R. A. L.

531. *Lightning Arresters.* **A. J. Wurts.** (J. Frank. Inst. 834. pp. 439–456, 1895.)—The author inclines to the view that overhead wires are more often charged by conduction from the atmosphere than either by a direct lightning-stroke or by static induction. The discharges from overhead wires seem to be oscillatory in character, and there are indications that electric waves are set up which combine and interfere with each other in such a manner as to produce frequent and unequally distributed points of high and low pressure. The ends of the line are points of reflection, and at these points the pressure is always great. Lightning arresters do not "protect" in the ordinary sense, they simply offer opportunities for discharge. These opportunities may or may not be embraced, according to circumstances, because there are formed along the line shifting points of high and low potential. The discharge therefore does not necessarily occur over the shortest or easiest path: that is, the discharge is selective; in consequence of which a liberal distribution of line arresters offers the only practical means of protecting widely distributed apparatus. The author mentions the function, the best form and dimensions of "choke-coils," when used for protecting apparatus against lightning. The Wurts' double-pole lightning arrester, for use on alternating circuits, is also described. Its distinctive feature is the employment of zinc for the spark electrodes; this metal is one of the five non-arcing metals. The form of arrester invented by Mr. Wurts for the protection of continuous-current circuits differs from that he uses for alternating circuits, and relies for its action on the fact that disruptive discharges take place comparatively easily over insulating surfaces as compared with discharges through air. The necessary precautions adopted to prevent the dynamo current from following the lightning discharge and forming a permanent arc are also described. W. G. R.

532. *Influence of Circular on Axial Magnetisation.* **I. Klemenčič.** (Wien. Ber. 104. Part II *a*. pp. 86–100, 1895.)—The magnetisation and hysteresis curves of thick iron and steel wires were taken, by the customary methods, when the wire was carrying a variable current. The mean circularly magnetising field thus produced varied from 0 to 15 units. It was found that circular magnetisation diminished the induction produced axially, but by an amount not proportional to the circular field nor proportional to the normal axial induction: the effect is greatest near the point of inflexion of the magnetisation curve, where it may amount to 75 per cent. diminution in soft iron and 25 per cent. in steel. The hysteresis curves are similarly flattened; the loss of energy in a cycle being slightly diminished, the retentivity much diminished (for iron) or slightly (for steel), while the coercive force is nearly unaltered. R. A. L.

## CHEMICAL PHYSICS.

533. *Determination of Isosmotic Concentrations.* **H. Köppe.** (Zschr. phys. Chem. 16. pp. 261–288, 1895.)—If a salt solution is mixed with blood, and the mixed liquid placed in a centrifugal machine, the red corpuscles can be separated. The ratio of the volume of these corpuscles to the whole volume of the blood depends on the osmotic pressure of the solution, decreasing as the osmotic pressure rises. In this way the osmotic pressure of a given solution can be compared with that of a standard solution such as potassium bichromate. The agreement with results obtained by De Vries and Hamburger is better than the agreement with those due to Raoult. The influence of dissociation is studied by comparing solutions of electrolytes with an indifferent solution like one of cane-sugar. W. C. D. W.

534. *Dilute Solutions and Osmotic Pressure.* **E. Bouty.** (J. Phys. 4. pp. 154–162, April 1895.)—The author considers that osmotic pressure is the result of an attraction between the solvent and the body dissolved, and represents the difference between the internal pressures inside the solution and the pure solvent. Beyond a certain dilution, further addition of solvent gives no thermal effect; and he shows, by considering the energy equation, that the specific heat of the dissolved body is then a function of the temperature only. If a solution of a gas is in equilibrium with the free gas in a cylinder, one of whose ends is permeable only to the liquid and the other only to the gas, the equilibrium will not be disturbed if we introduce a volume ($v$) of gas and a volume (V) of solvent just enough to dissolve it. By the general laws of equilibrium, we must then have

$$pv = PV,$$

where $p$ is the gaseous and P the osmotic pressure.

$$\text{But} \qquad pv = RT;$$

$$\therefore \qquad PV = RT,$$

the value of R being the same as for the same quantity of substance in the gaseous state. From Raoult's empirical laws of the vapour-pressures of solutions, the same equation is deduced for cases in which the dissolved body is non-volatile. W. C. D. W.

535. *Colour, Specific Gravity, and Surface-Tension of Hydrogen Peroxide.* **W. Spring.** (Zschr. anorg. Chem. 8. pp. 424–433, 1895.)—The author describes a method for the extraction of pure hydrogen peroxide from the commercial solution. The pure substance is a very strong explosive. The specific gravity of the liquid which contains 99·88 per cent. of $H_2O_2$ is 1·4998 at 15°; a result slightly higher than that found by Thenard. The ratio between the surface-tension of this liquid and that of water is 0·456 at 10°.

Diluting the hydrogen peroxide with water, the surface-tension increases rapidly at first and more slowly afterwards. The great difference between the surface-tensions of the two liquids may help to explain the decomposition of the $H_2O_2$ by finely divided metals, as gold and platinum. The colour of $H_2O_2$ is almost the same as that of water, but is deeper in the ratio of 1·83. In this comparison a 98 per cent. solution is used. The author points out that a blue colour is characteristic of oxygen, ozone, water, and hydrogen peroxide. S. S.

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536. *Petroleum as an Anti-Incrustator.* **G. Liévin.** (C. R. 120. p. 1134, 1895.)—On the addition of crude petroleum to the feed-water, a mud settled in the boilers which could easily be blown out. H. B.

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537. *Isotonic Solutions determined by Centrifugalising Blood-Mixtures.* **S. G. Hedin.** (Zschr. phys. Chem. 17. pp. 164-170, 1895.)—Equal volumes of blood and a salt solution (10 c.c.) are mixed in a test-tube, and part of the mixture is centrifugalised in a graduated thermometer-tube 70 mm. long. The volume of the blood-corpuscles is observed after 20 to 25 minutes whirling at about 6000 revolutions to the minute. It is found that (1) the same blood gives with the same salt solution the same volume of blood-corpuscles; (2) when different concentrations of the same salt are used, the volume is greater the more dilute the solution; (3) isotonic solutions of different salts give approximately the same volumes of blood-corpuscles. The results for 21 salts are compared with those of other observers using the same and different methods. S. S.

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538. *Molecular Weight of Iodine in Solution.* **E. Beckmann** and **A. Stock.** (Zschr. phys. Chem. 17. pp. 107–135, 1895.)—Many experiments have been made with iodine dissolved in various solvents, with the view of discovering whether the different colours of iodine solutions indicate changes in the molecular size. The authors reopen the question, as a correction should be made for the volatility of the iodine when using the boiling-point method. Applying such a correction to the results from the boiling-point determinations, they find that iodine exists as the molecule $I_2$ in carbon tetrachloride, chloroform, ethylene chloride, benzene, ethyl and methyl alcohols, methylal, and acetone. It forms a violet solution in the first two solvents, red in the second two, and brown in the remainder.—Returning to the freezing-point method, an explanation is needed of the abnormal values obtained when benzene is used. The authors show that a solid solution of iodine in benzene crystallises out, and not the pure solvent. Two methods are used: one in which both bromoform and iodine are dissolved in benzene, and the ratios of the quantities of the two substances are determined by analysis, both in the mother liquor and in the mass of separated crystals. It is found that there is a large accumulation

of iodine in the crystals. In the second method the crystals are separated from the mother liquor by a centrifugal machine. The concentration of the solid benzene solution is 0·357 the concentration of the liquid solution. The result for the molecular weight of iodine obtained by freezing a benzene solution should be multiplied by 0·643, and this gives the molecular weight of iodine between 224 and 231. Gautier and Charpy have stated that the different coloured solutions of iodine act in different ways upon lead amalgam, and have inferred from this that the iodine had not the same molecular state in the various solvents. The author shows that the result of the action depends on the length of time during which the amalgam is shaken with the iodine solution, and largely upon the solubility of mercuric iodide in the various solvents. So these experiments are not against the view that the molecule is $I_2$ in all the solvents which have been used. S. S.

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539. *Spectrochemistry of Nitrogen.* **J. W. Brühl.** (Zschr. phys. Chem. 16. pp. 193–225, 226–241, 497–511, and 512–524, 1895.)—I. The first of these four papers contains a list of the 132 nitrogen compounds investigated, with their properties, and how prepared. The $\mu^2$-constants established by L. Lorenz and H. A. Lorentz are used, *i. e.*,

Specific refractive power .... $\mathfrak{R}=(\mu^2-1)/(\mu^2+2)d$;
Molecular refractive power .. $\mathfrak{M}=(\mu^2-1)P/(\mu^2+2)d$.

Dispersive power is measured by the constants $\mathfrak{R}_\alpha-\mathfrak{R}_\gamma$ and $\mathfrak{M}_\alpha-\mathfrak{M}_\gamma$ introduced by the author. Table I. at the end gives the densities and refractive indices for various spectral lines, and Table II. gives the corresponding specific and molecular refractive powers.

II. In the second paper the results of the investigation of 58 isomeric nitrogen compounds are discussed. Only the specific refraction and dispersion are considered: for naturally with isomers the molecular constants give the same relations.

A. Saturated isomeric compounds do not possess the same refractive or dispersive powers; but the following general relations between various classes of such bodies are found to hold good:—(1) In the nitro-compounds the refractive and dispersive powers are smaller, but the boiling-point, density, and refractive index are greater than in the isomeric nitrites. (2) All these constants are much smaller in pyridine-derivatives than in any other isomers of the aromatic series. (3) The linkings $N\equiv C$ and $N=C=N$ are not equivalent in their effects upon refractive and dispersive powers. (4) The presence of ethylene-linkings exercises a most marked effect and increases the refractive and dispersive powers more than anything else.—B. Isomers of position may be divided into classes, some of which are iso-spectric (*i. e.* have the same, or nearly the same, refractive or dispersive powers), while others are hetero-spectric. (1) The isomeric ketoximes and aldoximes are

iso-spectric. (2) Of the amines which are isomeric in position, some are hetero-spectric and some iso-spectric. Amongst the latter are the "branch-isomeric" amines,—in which an equal number of equally-saturated carbon atoms branch off from the nitrogen stem, and the isomerism consists simply in a different grouping of the atoms of the branched radicles. (3) In nitrogen compounds of the aromatic series, the nitrogen may either be connected directly to the aromatic nucleus, or it may simply form part of the side-chain. Isomeric amines of this kind ("nucleus-isomers") are hetero-spectric. That isomer possesses the greater refractive and dispersive power in which the nitrogen is directly connected to the unsaturated radicle. (4) Primary amines have smaller refractive and dispersive powers, but greater density, refractive index, and boiling-point than isomeric secondary amines. The same holds good in comparing secondary amines with isomeric tertiary amines.

III. Spectrometric Constants of Nitrogen in the Amines.—These exhibit considerable variations, although the nitrogen is always present in them as a trivalent and saturated atom. Their value depends upon the kind of atoms with which the nitrogen is directly connected. They are constant for a given kind of combination, *e. g.*, that which occurs in the primary amines; and for this particular class of amine the constants are the same as those deduced from ammonia-gas and hydroxylamine. The following table gives for each kind of combination the mean value of the atomic refraction ($R_\alpha$) for the line $H_\alpha$, and also the atomic dispersion ($R_\gamma - R_\alpha$):—

| | $R_\alpha$. | $R_\gamma - R_\alpha$. |
|---|---|---|
| *Methane-amines.* | | |
| $H_2\mathbf{N}{-}C{-}$ ...... | 2·311 | 0·074 |
| $H\mathbf{N}({-}C{-})_2$ ...... | 2·604 | 0·135 |
| $\mathbf{N}({-}C{-})_3$ ...... | 2·924 | 0·191 |
| *Phenyl-amines.* | | |
| $H_2\mathbf{N}Bz.$ ......... | 3·016 | 0·624 |
| $H\mathbf{N}\langle^{Bz}_{C-}$ ........ | 3·408 | 0·815 |
| $\mathbf{N}^{Bz}_{({-}C{-})_2}$ ...... | 4·105 | 1·105 |
| *Phenyl-methane-amines.* | | |
| $H_2\mathbf{N}{-}C_n{-}Bz$ .... | 2·255 | 0·26 (ca.) |
| $H\mathbf{N}({-}C_n{-}Bz$ .. | 2·285 | 0·40 (ca.) |

Thus the atomic refraction and dispersion of nitrogen are smaller in the primary amines than in the secondary, and smaller in the secondary than in the tertiary; and this holds good for the piperidine and aromatic series as well as the fatty series.

IV. Spectrometric Constants of Nitrogen in the Cyanogen Compounds (Nitrils).—In the case of all the nitrils investigated, the atomic refraction for sodium light $R_{N_a}$ was found to be smaller than that for red hydrogen light $R_{\alpha}$. This appears to be characteristic of the cyanides: for most substances $R_{N_a}$ is greater than $R_{\alpha}$. The following table gives the mean values of the constants for nitrogen $N^{\equiv C-}$ in the nitrils in various forms of combination:—

| | $R_{\alpha}$. | $R_{\gamma}-R_{\alpha}$. |
|---|---|---|
| $N^{\equiv C-C}$ ........ | 3·176 | 0·084 |
| $N^{\equiv C-N}$ ........ | 2·995 | 0·00 |
| $N^{\equiv C'-Bz}$ ...... | 3·825 | 0·450 |

Thus the atomic refraction of nitril-nitrogen is much greater than that of nitrogen in the primary amines (see III.). It is even greater than that in the tertiary amines; although the atomic dispersion in the latter is more than twice that in the nitrils. The atomic dispersion in the nitrils and primary amines is about equal. The spectrometric properties of cyanogen gas show that it is most nearly allied to the phenyl-nitrils. Those of hydrocyanic acid (gas) show that it is entirely analogous to the alkyl-nitrils; so that if the structure R—C≡N is assigned to the latter, we must regard hydrocyanic acid as H—C≡N.

Spectrometric Constants of Nitrogen doubly-linked with Carbon. —The examination of a number of aldoximes and ketoximes gives for $^{O-}N^{=C}$ the following values:—

$$R_{\alpha}=3{\cdot}921, \quad R_{\gamma}-R_{\alpha}=0{\cdot}251.$$

In this kind of combination the atomic refraction of nitrogen is nearly twice as great as in the primary amines; and the dispersion is more than three times as great. It is remarkable that this double linkage is optically more powerful than the treble linkage in the nitrils.

D. E. J.

540. *Critical Temperature as a Test of Chemical Purity.* **R. Knietsch.** (Zschr. phys. Chem. 16. pp. 731–732, 1895.)—Pictet and Altschul (Zschr. phys. Chem. 16. p. 16) have shown that impurities in nitrous oxide influence its critical temperature; but the author, in Liebig's 'Annalen,' 259. p. 116, observed the same phenomenon in the case of chlorine. He therefore agrees with these observers in regarding the critical temperature determination comparable to that of a melting-point or boiling-point in ascertaining the purity or otherwise of a gaseous compound or element.

S. R.

# INDEX TO ABSTRACTS.

## GENERAL PHYSICS.


Page

484. Andrée's projected Balloon Expedition to the Pole. *G. Tissandier* ... 293
485. Effects of Expired Air upon Animal Life. *D. H. Bergey, S. W. Mitchell, & J. S. Billings* ... 293
486. Density of Helium. *Clève* ... 294
487. Abnormal Variations of Pressure with Height. *L. Teisserenc de Bort*. 294
488. New Method of Measuring the Freezing-Points of very Dilute Solutions. *A. Leduc* ... 295
489. Thermal Conductivity of Copper. *R. W. Quick, C. D. Child, & B. S. Lanphear* ... 295
490. Colour Relations of Atoms, Ions, and Molecules. *M. C. Lea* ... 295
491. The Geology of the Moon. *E. Suess* ... 296
492. Daily Movement of Wind Velocities in the United States. *F. Waldo* 299
493. Rise in Solubility. *H. Goldschmidt* ... 300
494. Automatic Recording Instrument for End Measurements. *L. Hartmann* ... 301
495. Permeability of Solid Bodies to the Ether. *L. Zehnder* ... 301
496. Relation between Relief and Seismological Frequency. *De Montessus* 303
497. Critical Temperature of Hydrogen. *L. Natanson* ... 303
498. Freezing of Solutions at Constant Temperatures. *A. Colson* ... 304
499. Closed Isothermal Reversible Cycle under Gravity. *A. Ponsot* ... 305


## LIGHT.


500. Fluted Spectra. *A. Schuster* ... 306
501. Errors in Photometric Standards. *E. Liebenthal* ... 306
502. Stereoscopic Projections and Lanterns. *Moëssard* ... 307
503. Spectroscopical Observations of Saturn's Rings. *H. Deslandres* ... 308
504. A Mirror Cœlostat. *G. Lippmann* ... 309
505. Dissociation and Optical Rotation. *E. Rimbach* ... 309
506. Infra-Red Dichroism. *E. Merritt* ... 310
507. Cobalt and Chromium Absorption-Bands. *A. Étard* ... 310
508. Photometry of Mean Spherical Power. *A. Blondel* ... 311
509. White Light. *A. Garbasso* ... 312


## HEAT.


510. Proof of the Boltzmann-Maxwell Law. *M. Planck*.—The Maxwell Law. *L. Boltzmann* ... 313
511. Some Relations between Temperature, Pressure, and Latent Heat of Vaporisation. *C. E. Linebarger* ... 313
512. Experimental Determination of the Density of Saturated Vapours. *G. Bauer* ... 314


[*Continued over-leaf.*

## INDEX (*continued*).

### HEAT (*continued*).


Page

513. Dilatation of Aqueous Solutions. *C. Forch* .......... 315
514. Measurement of High Temperatures with the Thermo-Element and the Melting-Point of some Inorganic Salts. *J. McCrae* .......... 315
515. Thermometer with a Constant Freezing-Point. *L. Marchis* .......... 315
516. Specific Heat of Mercury between 0° and 30°. *A. Bartoli & E. Stracciati* .......... 316


### ELECTRICITY.


517. Quadrant Electrometer as a Differential Instrument. *H. Eisler* .......... 317
518. New Method of Measuring Specific Inductive Capacity. *A. Nodon* .......... 317
519. Electric Discharge in Argon and Fluorine. *H. Moissan* .......... 317
520. Atmospheric Electricity on the Eiffel Tower. *Chauveau* .......... 317
521. Electricity Meter. *Harries & Riemann* .......... 318
522. Extremely Sensitive Galvanometer. *P. Weiss* .......... 318
523. Duration of Electrical Shadows caused by Solid and Fluid Insulators. *G. Quincke* .......... 319
524. Microscopy of Spark-Gaps. *G. T. Hanchett* .......... 319
525. Specific Inductive Capacities. *D. Mazzotto* .......... 320
526. Sparking Distance. *G. Jaumann* .......... 320
527. Capacity of Electrolytic Condensers. *S. Sheldon, H. W. Leitch, & A. N. Shaw* .......... 321
528. Resonance in Transformer Circuits. *F. Bedell & A. C. Crehore* .......... 322
529. Electrical Observations on the Sonnblick. *J. Elster & H. Geitel* .......... 323
530. Thermoelectric Power and Elongation of Magnetised Iron. *P. Bachmetjew* .......... 324
531. Lightning Arresters. *A. J. Wurts* .......... 324
532. Influence of Circular on Axial Magnetisation. *I. Klemenčič* .......... 325


### CHEMICAL PHYSICS.


533. Determination of Isosmotic Concentrations. *H. Köppe* .......... 326
534. Dilute Solutions and Osmotic Pressure. *E. Bouty* .......... 326
535. Colour, Specific Gravity, and Surface-Tension of Hydrogen Peroxide. *W. Spring* .......... 326
536. Petroleum as an Anti-Incrustator. *G. Liévin* .......... 327
537. Isotonic Solutions determined by Centrifugalising Blood-Mixtures. *S. G. Hedin* .......... 327
538. Molecular Weight of Iodine in Solution. *E. Beckmann & A. Stock* .......... 327
539. Spectrochemistry of Nitrogen. *J. W. Brühl* .......... 328
540. Critical Temperature as a Test of Chemical Purity. *R. Knietsch* .......... 330